# American Political Science Review

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# American Political Science Review

Volume 90, Number 3, September 1996

## Table of Contents

EDITOR'S NOTES ........ **viii**

ARTICLES

Political Inclusion and the Dynamics of Democratization
**John S. Dryzek** ........ **475**

Hume's Constitutionalism and the Identity of Constitutional Democracy
**Robert A. Manzer** ........ **488**

Serving God and Mammon: The Lockean Sympathy in Early American Political Thought
**Joshua Foa Dienstag** ........ **497**

Assessing the Dyadic Nature of the Democratic Peace, 1918–88
**David L. Rousseau, Christopher Gelpi, Dan Reiter, and Paul K. Huth** ........ **512**

The Costs of Coalition: Coalition Theories and Italian Governments
**Carol A. Mershon** ........ **534**

Dynamics of Cosponsorship
**Daniel Kessler and Keith Krehbiel** ........ **555**

The Dynamics of Aggregate Partisanship
**Janet M. Box-Steffensmeier and Renée M. Smith** ........ **567**

Strategic Policy Considerations and Voting Fluidity on the Burger Court
**Forrest Maltzman and Paul Wahlbeck** ........ **581**

RESEARCH NOTE

"Race Coding" and White Opposition to Welfare
**Martin Gilens** ........ **593**

FORUM

History, Historiography, and Political Science: Multiple Historical Records and the Problem of Selection Bias
**Ian S. Lustick** ........ **605**

BOOK REVIEWS

Political Theory

Ahrensdorf, Peter J. *The Death of Socrates and the Life of Philosophy.* See Parens, Joshua, below.
Barnes, Barry. *The Elements of Social Theory*
Giddens, Anthony. *Politics, Sociology, and Social Theory: Encounters with Classical and Contemporary Social Thought* ........ Daniel Little **619**
Berkowitz, Peter. *Nietzsche: The Ethics of an Immoralist*........ Bruce Detwiler **620**
Best, Steven. *The Politics of Historical Vision: Marx, Foucault, Habermas* ........ Stephen K. White **621**
Cooper, Davina. *Power in Struggle: Feminism, Sexuality, and the State* ........ Susan Bickford **622**

*Cropsey, Joseph. *Plato's World: Man's Place in the Cosmos* .......... Christopher Bruell **623**
Eisenberg, Avigail I. *Reconstructing Political Pluralism* .................. Dennis Bathory **624**
Fishkin, James S. *The Voice of the People: Public Opinion and Democracy* .................. Robert Y. Shapiro **625**
Fitzgibbons, Athol. *Adam Smith's System of Liberty, Wealth, and Virtue: The Moral and Political Foundations of* The Wealth of Nations .................. Roger J. Fechner **626**
Giddens, Anthony. *Politics, Sociology, and Social Theory: Encounters with Classical and Contemporary Social Thought.* See Barnes, Barry, above.
Grasso, Kenneth L., Gerard V. Bradley, and Robert P. Hunt, eds. *Catholicism, Liberalism, and Communitarianism: The Catholic Intellectual Tradition and the Moral Foundations of Democracy* .................. Simone Chambers **627**
Honig, Bonnie, ed. *Feminist Interpretations of Hannah Arendt* ............ Jeffrey C. Isaac **628**
Horowitz, Asher, and Terry Maley, eds. *The Barbarism of Reason: Max Weber and the Twilight of Enlightenment* .................. David Sciulli **629**
Jones, Peter. *Rights* .................. Jack Donnelly **631**
Kagel, John H., and Alvin E. Roth, eds. *The Handbook of Experimental Economics* .................. Kenneth C. Williams **632**
Lampert, Laurence. *Leo Strauss and Nietzsche* .................. Edward Andrew **633**
Lorber, Judith. *Paradoxes of Gender* .................. Terrell Carver **634**
Miller, David, and Michael Walzer, eds. *Pluralism, Justice, and Equality* .................. Avigail I. Eisenberg **635**
Nussbaum, Martha. *Poetic Justice: The Literary Imagination and Public Life* .................. Francis A. Beer **636**
Palmer, Michael, and Thomas L. Pangle, eds. *Political Philosophy and the Human Soul: Essays in Honor of Allan Bloom* .................. Fred M. Frohock **637**
Parens, Joshua. *Metaphysics as Rhetoric: Alfarabi's Summary of Plato's Laws*
  Rosen, Stanley. *Plato's* Statesman*: The Web of Politics*
  Ahrensdorf, Peter J. *The Death of Socrates and the Life of Philosophy* .................. Mark J. Lutz **638**
Rosen, Stanley. *Plato's* Statesman*: The Web of Politics.* See Parens, Joshua, above.
Schwartz, Joseph M. *The Permanence of the Political: A Democratic Critique of the Radical Impulse to Transcend Politics* .................. Benjamin Barber **640**
Weinstein, Michael A. *Culture/Flesh: Explorations of Postcivilized Modernity* .................. Nancy S. Love **641**
Yack, Bernard, ed. *Liberalism without Illusions: Essays on Liberal Theory and the Political Vision of Judith N. Shklar* ......... Richard E. Flathman **642**

American Politics

Barber, Kathleen L. *Proportional Representation and Election Reform in Ohio* .................. V. Jerone Stephens **643**
Brennan, Mary C. *Turning Right in the Sixties: The Conservative Capture of the GOP* .................. William F. Connelly, Jr. **643**
Campbell, Ballard C. *The Growth of American Government: Governance from the Cleveland Era to the Present* .................. Charles T. Goodsell **644**
Campbell, Colin, and Bert A. Rockman, eds. *The Clinton Presidency: First Appraisals* .................. Norman C. Thomas **645**
Finegold, Kenneth. *Experts and Politicians: Reform Challenges to Machine Politics in New York, Cleveland, and Chicago* .............. Clarence N. Stone **646**
Friedman, Barry D. *Regulation in the Reagan-Bush Era: The Eruption of Presidential Influence* .................. Richard W. Waterman **647**
Gangi, William. *Saving the Constitution from the Courts* .............. Gary L. McDowell **648**
Gormley, William T., Jr. *Everybody's Children: Child Care as a Public Problem* .................. Jane Waldfogel **649**
Hacker, Kenneth L., ed. *Candidate Images in Presidential Elections* .................. Mary E. Stuckey **650**
Handberg, Roger. *The Future of the Space Industry: Private Enterprise and Public Policy* .................. Nathan C. Goldman **651**

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*Reprinted due to significant errors in the version printed in the March 1996 issue.

Ingraham, Patricia Wallace. *The Foundation of Merit: Public Service in American Democracy* ........ Peri E. Arnold **652**
Kay, W. D. *Can Democracies Fly in Space? The Challenge of Revitalizing the U.S. Space Program* ........ Radford Byerly, Jr. **653**
Lavrakas, Paul J., Michael W. Traugott, and Peter V. Miller, eds. *Presidential Polls and the News Media* ........ S. Robert Lichter **654**
Lodge, Milton, and Kathleen McGraw, eds. *Political Judgement: Structure and Process* ........ James H. Kuklinski **654**
Mayer, William G., ed. *In Pursuit of the White House: How We Choose Our Presidential Nominees* ........ Ryan J. Barilleaux **655**
Navarro, Armando. *Mexican American Youth Organization: Avant Garde of the Chicano Movement in Texas* ........ Benjamin Marquez **656**
Roe, Emery. *Narrative Policy Analysis: Theory and Practice* ........ Charles Lester **657**
Schneider, Saundra K. *Flirting with Disaster: Public Management in Crisis Situations* ........ Louise K. Comfort **658**
Sclove, Richard E. *Democracy and Technology* ........ Sheila Jasanoff **659**
Soder, Roger, ed. *Democracy, Education, and the Schools* ........ William A. Galston **660**
Stein, Robert M., and Kenneth N. Bickers. *Perpetuating the Pork Barrel: Policy Subsystems and American Democracy* ........ Gerald Strom **661**
Van Beek, Stephen D. *Post-Passage Politics: Bicameral Resolution in Congress* ........ Walter J. Oleszek **662**
Whiteman, David. *Communication in Congress: Members, Staff, and the Search for Information* ........ Bruce Bimber **662**
Wittman, Donald. *The Myth of Democratic Failure: Why Political Institutions Are Efficient* ........ Barry R. Weingast **663**
Yarnold, Barbara M. *Abortion Politics in the Federal Courts: Right versus Right* ........ Laura R. Woliver **664**

Comparative Politics

Abramson, Paul R., and Ronald Inglehart. *Value Change in Global Perspective* ........ Raymond M. Duch **665**
Bell, Daniel A., David Brown, Kanishka Jayasuriya, and David M. Jones. *Towards Illiberal Democracy in Pacific Asia* ........ Ross Marlay **666**
Casper, Gretchen. *Fragile Democracies: The Legacies of Authoritarian Rule* ........ Belinda A. Aquino **667**
Clarke, Simon, Peter Fairbrother, and Vadim Borisov. *The Workers' Movement in Russia*
  Filtzer, Donald. *Soviet Workers and the Collapse of Perestroika: The Soviet Labour Process and Gorbachev's Reforms, 1985–1991* ........ Darrell Slider **668**
Evans, Peter. *Embedded Autonomy: States and Industrial Transformation* ........ Atul Kohli **669**
Farnen, Russell F. *Nationalism, Ethnicity, and Identity: Cross-National and Comparative Perspectives* ........ William Safran **670**
Filtzer, Donald. *Soviet Workers and the Collapse of Perestroika: The Soviet Labour Process and Gorbachev's Reforms, 1985–1991.* See Clarke, Simon, Peter Fairbrother, and Vadim Borisov, above.
Gilbert, Mark. *The Italian Revolution: The End of Politics Italian Style?*
  McCarthy, Patrick. *The Crisis of the Italian State: From the Origins of the Cold War to the Fall of Berlusconi* ........ Leonard Weinberg **670**
Gunther, Richard, P. Nikiforos Diamandouros, and Hans-Jürgen Puhle, eds. *The Politics of Democratic Consolidation: Southern Europe in Comparative Perspective* ........ Enrique A. Baloyra **672**
Hopkins, Jack W. *Policymaking for Conservation in Latin America: National Parks, Reserves, and the Environment* ........ Eduardo Silva **673**
Keren, Michael. *Professionals against Populism: The Peres Government and Democracy* ........ Jerome Slater **674**
Kluegel, James R., David S. Mason, and Bernard Wegener, eds. *Social Justice and Political Change: Public Opinion in Capitalist and Post-Capitalist States* ........ Euel Elliott **676**
Marklew, Victoria. *Cash, Crisis, and Corporate Governance: The Role of National Financial Systems in Industrial Restructuring* ........ Nikolaos Zahariadis **677**
Marty, Martin E., and R. Scott Appleby, eds. *Fundamentalism Comprehended* ........ David C. Rapoport **678**

McCarthy, Patrick. *The Crisis of the Italian State: From the Origins of the Cold War to the Fall of Berlusconi.* See Gilbert, Mark, above.
Moghadam, Valentine M., ed. *Democratic Reform and the Position of Women in Transitional Economies* ................ Barbara Jancar-Webster **679**
Petro, Nicolai N. *The Rebirth of Russian Democracy: An Interpretation of Political Culture* .................................... Archie Brown **680**
Schulz, Donald E., and Edward J. Williams, eds. *Mexico Faces the 21st Century* .................................................. Joseph L. Klesner **681**
Shafir, Gershon. *Immigrants and Nationalists: Ethnic Conflict and Accommodation in Catalonia, the Basque Country, Latvia, and Estonia* ................................................... Milton J. Esman **682**
Shambaugh, David, ed. *Deng Xiaoping: Portrait of a Chinese Statesman* ................................................... Quansheng Zhao **683**
Tate, C. Neal, and Torbjörn Vallinder, eds. *The Global Expansion of Judicial Power* .................................... Jerry Waltman **684**
Tismaneanu, Vladimir, ed. *Political Culture and Civil Society in Russia and the New States of Eurasia* ............................... Russell Bova **685**
Van Cott, Donna Lee, ed. *Indigenous Peoples and Democracy in Latin America* ................................................ Eric Selbin **686**
Volgyes, Ivan, and Zoltan Barany, eds. *The Legacies of Communism in Eastern Europe* ................................ Michael Bernhard **687**
von Mettenheim, Kurt. *The Brazilian Voter: Mass Politics in Democratic Transition, 1974–1986* ............................ Anthony W. Pereira **688**
Warwick, Paul V. *Government Survival in Parliamentary Democracies* ................................................ Kaare Strøm **689**

International Relations

Bose, Christine E., and Edna Acosta-Bélen, eds. *Women in the Latin American Development Process* .................................. Rita Jalali **690**
Cimbala, Stephen J. *Collective Insecurity: U.S. Defense Policy and the New World Disorder* .................................. William C. Martel **691**
Dawisha, Adeed, and Karen Dawisha, eds. *The Making of Foreign Policy in Russia and the New States of Eurasia.*
Nichol, James P. *Diplomacy in the Former Soviet Republics* ............. Glenn Chafetz **692**
Downs, George W., and David M. Rocke. *Optimal Imperfection? Domestic Uncertainty and Institutions in International Relations* ........................................ T. Clifton Morgan **693**
Esman, Milton J., and Shibley Telhami, eds. *International Organizations and Ethnic Conflict* ............................. Manus I. Midlarsky **694**
Fischer, Frank, and Michael Black, eds. *Greening Environmental Policy: The Politics of a Sustainable Future*
Rowlands, Ian H. *The Politics of Global Atmospheric Change* ........... Karen T. Litfin **695**
Harris, Nigel. *The New Untouchables: Immigration and the New World Worker* ............................................ Anthony M. Messina **697**
Iyob, Ruth. *The Eritrean Struggle for Independence: Domination, Resistance, Nationalism, 1941–1993* ............................... Edmond J. Keller **698**
Kolstoe, Paul. *Russians in the Former Soviet Republics* .................. Rasma Karklins **699**
Kroll, John A. *Closure in International Politics: The Impact of Strategy, Blocs, and Empires* .................................. Carolyn Rhodes **700**
Miller, Marian A. L. *The Third World in Global Environmental Politics* ............................................ Amitrajeet A. Batabyal **701**
Nichol, James P. *Diplomacy in the Former Soviet Republics.* See Dawisha, Adeed, and Karen Dawisha, above.
Payne, Richard J. *The Clash with Distant Cultures: Values, Interests and Force in American Foreign Policy* ....................... David Campbell **702**
Ray, James Lee. *Democracy and International Conflict: An Evaluation of the Democratic Peace Proposition* ..................... William J. Dixon **703**
Ross, George. *Jacques Delors and European Integration* ................... Leon Hurwitz **704**
Rowlands, Ian H. *The Politics of Global Atmospheric Change.* See Fischer, Frank, and Michael Black, above.
Schneider, Gerald, Patricia Weitsman, and Thomas Bernauer, eds. *Towards a New Europe: Stops and Starts in Regional Integration* .................................................. Robert Leonardi **705**
Snider, Don M., and Miranda A. Carlton-Carew, eds. *U.S. Civil-Military Relations: In Crisis or Transition?* ......................... Sam C. Sarkesian **706**

Terriff, Terry. *The Nixon Administration and the Making of U.S. Nuclear Strategy* . . . . . . . . . . . . . . . . Eric Mlyn **707**
Walton, John, and David Seddon. *Free Markets and Food Riots: The Politics of Global Adjustment* . . . . . . . . . . . . . . . . Joan M. Nelson **708**
Waltz, Susan E. *Human Rights and Reform: Changing the Face of North African Politics* . . . . . . . . . . . . . . . . Lisa Anderson **709**
Yetiv, Steve A. *America and the Persian Gulf: The Third Party Dimension in World Politics* . . . . . . . . . . . . . . . . K. A. Beyoghlow **710**
Zartman, I. William, ed. *Elusive Peace: Negotiating an End to Civil Wars* . . . . . . . . . . . . . . . . Thomas G. Weiss **711**
Zelikow, Philip, and Condoleezza Rice. *Germany Unified and Europe Transformed: A Study in Statecraft* . . . . . . . . . . . . . . . . Andrei S. Markovits **712**

FORTHCOMING ARTICLES

Do Majority–Minority Districts Maximize Substantive Black Representation in Congress?, **Charles Cameron, David Epstein, and Sharyn O'Halloran**
Do Bills of Rights Matter?, **Charles R. Epp**
The Claim of Issue Creation on the U.S. Supreme Court, **Lee Epstein, Jeffrey A. Segal, and Timothy Johnson**
Unequal Participation: Democracy's Unresolved Dilemma, **Arend Lijphart**
The Electoral Connection in the Chinese Countryside, **Melanie F. Manion**
The European Parliament as a Conditional Agenda Setter: What Are the Conditions? A Critique of Tsebelis (1994), **Peter Moser**
Appeasement as a Game of Timing, **Robert Powell**
Trends in the Partisan Composition of State Legislatures: A Response to Fiorina, **Jeffrey M. Stonecash and Anna M. Agathangelou**
Voting and Vetoing, **Eyal Winter**

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The vast majority of papers submitted to the *APSR* go smoothly through the review process, but occasionally processing is delayed by preventable problems that are worth calling to the attention of prospective authors.

The most difficult submission problems involve manuscripts that present potential conflicts with the *Review*'s policy against duplicate or overlapping submissions or publication (see Instructions to Contributors). We want to publish the best work that is *not* also appearing in other venues. Most journals have similar policies, and nonduplication of submissions is a general norm of scholarly publication. Observance of this norm also serves our collective interests by making space available to the largest number of authors and ideas possible, especially since the *APSR* can publish only about 9–10% of articles submitted.

Information about possible duplicate or overlapping submissions frequently comes to us from a reviewer, either as a result of having examined an author's book manuscript for a publisher or on the basis of detailed knowledge of research in the area. In the case of submissions potentially overlapping with books, authors are always asked about the accuracy of these reports and, if accurate, the anticipated publication date of the book. Journal publication prior to the publication of a book on the same subject is common and is recognized as a legitimate stage in scholarly production. If a book is scheduled to be published prior to the earliest time an article covering the same material could appear in print, however, or only shortly afterward, the article cannot be considered. Indeed, the guidelines stipulate that once a larger manuscript has been submitted to a publisher, derivative papers should not be sent to the *Review*. These situations can be frustrating. Work already accepted for book publication is frequently of a very high quality, and the editor would like to publish it almost as much as the author would. But where the timing is wrong, and the basic analysis is, or shortly will be, available elsewhere, it is inappropriate and unfair to ask for reviewer and editorial time to be invested in a manuscript while many lacking other publishing opportunities wait. These difficulties can be avoided if authors working on monographs will plan their article submissions for as early as possible in the process.

Related problems are presented by manuscripts that are only slight variations on a theme of other articles already published by the author or forthcoming elsewhere. We occasionally come across such articles in the in-house research we do of the scholarly literature for reviewer selection purposes. We have also received a few articles that included extensive amounts of text identical to or only slightly paraphrased from material in a prior publication of the author, without appropriate acknowledgment to the prior source. In a few cases where authors have submitted articles with titles, abstracts, or significant amounts of text very similar to those of papers forthcoming or already published elsewhere, I have asked them to explain the difference between the submitted article and the other publications. We weigh carefully an article's additional contributions to knowledge, and in some cases submissions that appeared very similar to prior research were, in fact, valuable extensions of it. At the other extreme, in a number of these cases, the amount of duplicated material included in a submission has been so great that publication in the *APSR* would clearly have violated the copyright of the other publisher.

In addition to the usual kinds of related articles, authors have asked us to consider English translations of materials that have already been published in another language, chapters in books on important topics about which the *APSR* publishes relatively little, and chapters in books whose audience is much narrower than the audience of the *Review*. However, the *Review* does not reprint articles or chapters already published elsewhere or whose copyright is owned by another publisher.

Few of these situations are cut and dried, and occasionally grey areas where journals have different norms can lead to misunderstandings. In one recent case, a submitted paper was sent by the *APSR* to a reviewer who, as luck would have it, had previously reviewed the same paper for another journal. Since the reviewer had been informed by the first editor that the paper was a "revise and resubmit," he charged that the author had engaged in duplicate submission, and the other editor concurred. However, in his "revise and resubmit" letter the other editor said that the paper would be reconsidered only if it underwent "drastic revision." Rather than following this course, the author submitted the paper to the *APSR*, and described his submissions as "serial" rather than "duplicate." Nevertheless, both the reviewer and the other editor felt that the paper remained "under consideration" at the other journal, and should not have been submitted elsewhere until the author formally withdrew it from further consideration there. At the *APSR*, in contrast, an invitation to resubmit implies that the paper is no longer being considered here until and unless a resubmission is received, so authors who receive such letters may submit elsewhere if they prefer to do so.

Nonanonymous submissions also cause problems. Since we use a double-blind review process, we ask that four of the five copies of a submission omit identifying references. Nevertheless, a substantial minority of manuscripts are not submitted in anonymous format. We have received papers with authors' names in runningheads, with first-person descriptions of and citations to the author's prior research, with thanks to colleagues in the author's department embedded in footnotes, and other subtle and not so subtle clues to authorship. One recent arrival was accompanied by a letter in which the author stated that the manuscript copies he had submitted were not anonymous because any knowledgeable reviewer would recognize his work anyway. The author was, indeed, a prominent scholar. But given research demonstrating that perceived author characteristics affect reader evaluations of manuscripts, it is particularly inappropriate to relax enforcement of the norm of anonymous submission for well-known scholars.

This is not to say that the authorship of a properly prepared anonymous paper will never be recognized. Even when papers conform to the anonymity requirement, knowledgeable reviewers will often recognize their authors, particularly where the author has significant prior publications on the subject or has presented the paper at a professional meeting, distributed it over the Internet, or circulated it widely among colleagues prior to submission. Reviewers know the norms, however, and try to comply with the double-blind goal. They sometimes decline to review a paper because they believe they know who the author is or ask whether they should review it. Some reviewers tell me when they send in a review that they do know the author, but, since they felt they could be impartial, they reviewed the paper anyway. Particularly where an author has significant prior publications on related subjects, one obviously does not want reviewers who are unaware of them. But as long as we strive for a double-blind review process, the goal should be to make identification harder rather than easier. By omitting clues to authorship to the extent possible, an author also minimizes delays in turn-around time that occur when potential reviewers decline to review nonanonymous papers.

We frequently cut isolated identifying references from manuscripts, but if they are sprinkled throughout a paper, the author will be asked to resubmit in anonymous form. If references to an author's prior work are not central to the submission, omitting them is obviously the easiest way to mask authorship; if this is undesirable or would make the author appear ignorant of prior research, in almost all cases one can simply refer to this work in the third person. Where repeated references are required and third person is unworkable, an alternative may be to refer to "Author's work," and omit the references. (In that case, please do not leave citations in the list of references while changing the author's actual name to "Author.")

More often than one might suppose we receive only one copy of a manuscript or flawed copies, with pages out of order or pages, tables, references, or other parts of the manuscript missing or unreadable. Since one copy of each manuscript is read for reviewer selection purposes, if all copies are similarly afflicted, we will discover the problem and, if necessary, contact the author to resolve it. Occasionally, however, the copy we review is fine but reviewers later report that their copies are missing pages, illegible, or have other problems that prevent the review from being completed.

Excessive manuscript length can also lead to delays. The maximum permitted length is now 45 pages, but we have received manuscripts up to 70 pages long. Sometimes authors acknowledge a problematic length but cite the importance, novelty, comprehensiveness, or difficulty of researching the subject of their papers as reasons for an exception to be made, or ask for reviewers' suggestions for cutting. Occasionally, I have been persuaded to send lengthy papers out for review but almost always to both my and the authors' subsequent regret. Many reviewers object to being sent very long manuscripts and consider their authors to have violated the norms by submitting them and the editor to have done so by having sent them out for review. At best, papers that exceed the length guidelines may be delayed in the editorial office by my request for a shorter version or may languish on reviewers' desks for want of sufficient time; at worst, a reviewer may read the paper resentfully and subject it to unrealistically lofty standards. Most reviewers are very generous in the time and care they devote to manuscripts, but few define their job to include assistance to the author in editing a paper down to article length. Authors do themselves a disservice by submitting excessively long papers.

Despite these occasional problems, it is of course gratifying that so many authors think of the *Review* as the site in which they would like their work to be published. We are happy to answer any questions about submissions or other aspects of our procedures and committed to providing as fair and expeditious a review process as we can.

Ada W. Finifter

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# Political Inclusion and the Dynamics of Democratization

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*Once universal adult citizenship rights have been secured in a society, democratization is mostly a matter of the more authentic political inclusion of different groups and categories, for which formal political equality can hide continued exclusion or oppression. It is important, however, to distinguish between inclusion in the state and inclusion in the polity more generally. Democratic theorists who advocate a strategy of progressive inclusion of as many groups as possible in the state fail to recognize that the conditions for authentic as opposed to symbolic inclusion are quite demanding. History shows that benign inclusion in the state is possible only when (a) a group's defining concern can be assimilated to an established or emerging state imperative, and (b) civil society is not unduly depleted by the group's entry into the state. Absent such conditions, oppositional civil society may be a better focus for democratization than is the state. A flourishing oppositional sphere, and therefore the conditions for democratization itself, may actually be facilitated by a passively exclusive state, the main contemporary form of which is corporatism. Benign inclusion in the state can sometimes occur, but any such move should also produce exclusions that both facilitate future democratization and guard against any reversal of democratic commitment in state and society. These considerations have substantial implications for the strategic choices of social movements.*

## DEMOCRATIZATION AS INCLUSION

If democracy is a good thing (as almost everyone everywhere now seems to believe), then more democracy should presumably be an even better thing. Today there is a widespread sense, among political theorists at least, that democracy is an unfinished project, not just in terms of the spread of liberal democratic institutions to more and more corners of the world but also in terms of deepening of the democratic qualities of all societies.[1] Democratization in this latter sense is largely (though not solely) a matter of the progressive inclusion of various groups and categories of people in political life.[2] With the struggle for universal adult citizenship rights now more or less over in the industrial democracies, the attention of theorists and activists alike has turned to how democracy might be made more substantial and effective through greater efforts to include a variety of disadvantaged categories and groups for which the formal promise of democratic equality has masked continued exclusion or oppression (see Phillips 1995). Candidates for such inclusion are ethnic and religious minorities, indigenous peoples, women, the old, gays and lesbians, youth, the unemployed, the underclass, recent immigrants, people exposed to environmental risks, and (if only by proxy) future generations. With the demise of socialism and its often exclusive emphasis on the emancipation of the working class, such inclusive democratic endeavors are especially attractive to many on the left (e.g., Laclau and Mouffe 1985).

While recognizing that the effective inclusion of more groups and categories in the polity is central to democratization, I want to question any predisposition toward inclusion sponsored by, or sought in, the state, which may be defined as the set of individuals and organizations legally authorized to make binding decisions for a society. Entry into the state can come through organization as an interest group and associated lobbying activities; participation in policy development and implementation through ongoing negotiation between group leaders and public officials; participation in conventional party and electoral politics, either by organizing as a party or in formal affiliation with an established party; acceptance of governmental appointments by group leaders; or enhancing the group's ability to participate in policymaking through changes in public policy. This sort of inclusion or entry into the state therefore involves more than the attainment of basic citizenship rights, such as the right to vote and associate, which I shall take for granted. I argue that inclusion in the state is only benign if some fairly demanding criteria are met, and that when these criteria are not met, inclusion in the polity beyond the state is more appropriate.

Democratic theorists have, of course, always assumed that the state is the main locus of their concerns; indeed, this assumption is so universal and unremarkable that it is not often noted. Robert Dahl (1989, 37) makes a rare explicit acknowledgment: "Advocates of the democratic process have always meant it to be applied to the state." I will suggest that such an exclusive focus is often inappropriate, at least in the developed liberal democracies. Much of the time we should look instead to the polity beyond the state. In the past, the main nonstate focus of democratic theorists was the workplace. More recently, some attention has been paid to civil society (see Cohen and Arato 1992),[3] though that sphere remains both contested and ambiguous, as does its relationship to the state. Here I wish to both clarify and advance this emphasis on civil society.

Wariness of political inclusion is common in the ranks of conservatives and classical liberals. Conservatives

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[1] Moreover, warnings about the "democratic distemper" accompanying excessive public demands upon the state are less frequent today than they were two decades ago (i.e., Crozier, Huntington, and Watanuki 1975).

[2] This is not all democratization can mean. Within a society, it can also entail an increase in the scope (or range) of issues brought under popular control, or an increase in the authenticity of that control.

[3] Attention has also focused on the international system, though that is beyond my scope here.

want to repel destabilizing threats to the established order, and liberals want to protect their ideal of universal rights assigned equally to individuals irrespective of their characteristics, rather than to particular categories or groups. My own argument for highly selective inclusion is different in that it proceeds from the point of view of democratization itself. It is important to distinguish between inclusion in politics and inclusion in the state. Democrats should generally favor a state that is exclusive in important aspects, for exclusion properly arranged can benefit democracy and democratization, even from the point of view of those excluded. (That exclusion can do so from the point of view of those included has been a staple of republican thinking through the ages, from antiquity to Arendt [1958].) In some circumstances, inclusion in the state is defensible, so it becomes important to determine whether these circumstances obtain for particular groups facing the possibility of inclusion in particular contexts.

An examination of the history of democratization indicates that pressures for greater democracy almost always emanate from oppositional civil society, rarely or never from the state itself. If a group leaves this oppositional sphere to enter the state, then dominant classes and public officials have less to fear in the way of public protest. There may be some democratic gain in this entry, but there is also democratic loss in terms of a less vital civil society, the erosion of some existing democratic accomplishments, and a reduced likelihood of further democratization in the future. Moreover, the democratic gain is itself uncertain. I will argue that such gain can only be secured when the defining interest of the entering group can be connected quite directly to an existing or emerging state imperative. This connection can be made when an equivalence is discovered between the goods sought by a group and some aspect of what the state must do in terms of public policy. If the group's interest cannot be so assimilated, then the group in question receives only symbolic rewards. Such co-option has been a standing concern of observers of the role of groups in democratic systems. Co-option was defined long ago by Selznick (1966, 13) as "the process of absorbing new elements into the leadership or policy-determining structure of an organization as a means of averting threats to its stability or existence," though in the normal pejorative sense in which co-option is understood, such absorption comes without any real power sharing.[4]

The dynamics of democratization reveal a subtle interplay between inclusion and exclusion, the state and civil society. I will conclude with some criteria that help to determine whether any particular group's inclusion in the state constitutes a democratic gain or loss for the group in question and, more importantly, for the polity as a whole. Let me begin by exploring in more detail some of the implicitly or explicitly statist arguments that have been advanced on behalf of inclusion.

## THE INCLUSIVE STATE

Among the most visible proposals for democratic inclusion are those now advanced by "difference democrats," many of whom are also feminists. According to difference democrats, people are different from one another in fundamental ways, and to treat them as similar—e.g., by granting them all the same formal rights and the same access to politics—effectively extinguishes any political manifestation of difference (Young 1989, 1990). For example, indigenous peoples in Canada, the United States, or Australia are enfranchised under "one person, one vote." But given their numbers and geographical dispersal, this effectively denies them any representation in national legislatures. New Zealand has long recognized this problem and has set aside a number of Maori seats in parliament.[5] This does not mean that electoral reform is a sufficient remedy, for many other aspects of political systems make it difficult for disadvantaged groups to attain real recognition and access, even when these groups are large.

To some difference democrats, notably Anne Phillips (1993, 96–9), these considerations suggest only such measures as setting aside quotas of seats in parliament for particular categories of people, such as women. Political parties in Scandinavia and (recently) Britain have adopted quota systems of just this kind. Phillips is otherwise keen to preserve the basic structure of liberal democracy. She does not suggest that the representatives in question have a special charge to speak for women and only as women; it is enough that they simply *are* women.

Iris Young draws more radical inferences from the principle of difference. She finds the idea of universal citizenship repressive, as it does not recognize experiences and interests different in kind from those already dominant. Apparently universal rights and principles turn out, on closer inspection, to be tailored to the interests of upper-class white males. Young believes that universality erases everything that is particular, different, and valuable in the experience and identity of oppressed groups. Such groups should therefore retain their solidarity and distinctive identity in their representation and in their treatment by public policy. Historically oppressed groups which, in the United States, she identifies as "women, blacks, Native Americans, old people, poor people, disabled people, gay men and lesbians, Spanish-speaking Americans, young people, and nonprofessional workers" (Young 1989, 265) should be guaranteed not just representation in the legislature but also veto power over policies that affect them, and guarantees that public officials will respond to their concerns. In other words, the group should be represented qua group, rather than merely electing individual representatives with the characteristics of the group.

Now, the idea that the representation of groups rather than individuals should be the locus of democratic politics is not unique to contemporary difference democrats. Pluralists have always interpreted state-related politics in terms of the interaction of groups, and public policy as the output of that interaction. Midcentury U.S. pluralists such as David Truman (1951) and Robert Dahl (1956) may not have had the same set of interests in mind as do the difference democrats, but they shared their emphasis on different experiences producing different interests, which should then be pursued by interest organization through groups.[6]

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[4] For a detailed analysis of the hazards of co-option, especially for resource-poor groups, see Saward (1992).

[5] One parliamentary seat in the German *land* of Schleswig-Holstein is reserved for the tiny Danish minority.

These pluralists saw the state as passive, reacting to whatever groups happened to emerge. Given that they recognized few barriers to the emergence of interests and the organization of groups, they saw no need for public authority to intervene to affect the pattern of group representation (although they had no objection to the removal of barriers to group assertiveness, through, for example, civil rights legislation in the United States). In contrast, difference democrats see a variety of barriers to the emergence, recognition, organization, and assertiveness of groups. These barriers come mostly in the form of hierarchy and oppression, with cultural and economic as well as political causes. Whereas midcentury U.S. pluralism is passively inclusive in its pattern of representation in that it is prepared to accept whatever constellation of groups emerge from society to enter politics, difference democrats are more attuned to the need for actively inclusive representation, in which efforts are made to promote the ability of groups to recognize an interest and pursue it in politics. In this respect, they reveal a surprising affinity with James Madison's view of groups, or factions, as he would have called them; for Madison too did not believe that a desirable pattern of group representation emerges automatically.

Difference democrats are not always clear as to who does the recognizing and promoting. Cohen and Rogers (1992) are more forthright in their answer: the state. Sharing with both Madison and difference democrats the recognition that desirable patterns of interest organization and representation do not arise automatically (p. 426), they believe that the state should play an active role in sponsoring and certifying groups, removing obstacles to their exercising political influence, and creating channels for that influence to be felt in government. In particular, inequality of representation in their "associative democracy" should be remedied by state promotion of the organization of disadvantaged groups (p. 425). The kinds of associations Cohen and Rogers have in mind are mostly economic ones, especially categories of workers. Their emphasis on the economic basis of interest formation is shared by midcentury pluralists, though the latter were not, Dahl excepted, particularly interested in the working class.

State-sponsored association is proposed by Michael Walzer as an antidote to a rampant individualism that is producing "dissociated individuals" who are easy prey for antidemocratic demagogues (Walzer 1994, 189).[7] In the United States at least, Walzer (1991, 125) believes conditions have deteriorated to the extent that "it makes sense to call the state to the rescue of civil society." Walzer recommends government sponsorship and subsidy for trade unions, for "cultural associations," defined in ethnic or religious terms, that provide welfare, education, and health services, for "charter schools" designed and managed by parents and teachers, for tenants' housing cooperatives, for workers' cooperatives, and for a wide range of community projects (1994, 189). Group life enhanced along these lines will, Walzer hopes, produce efficacious and tolerant citizens, but it requires "certain background or framing conditions that can only be provided by state action" involving "a political strategy for mobilizing, organizing, and, if necessary, subsidizing the right sort of groups" (p. 191).

Once mobilized, Walzer's "right sort of groups" are left free to participate in politics without further state support. In contrast, Cohen and Rogers believe such support should continue through the creation of channels and mechanisms enabling groups to influence public policy. With this continuation, the state–sponsored pattern of representation advocated in the associative democracy of Cohen and Rogers does, as they recognize, have more in common with corporatism than with liberalism.[8] Corporatism as a form for the organization of national political systems may be defined as a tripartite concertation of government, labor, and business, the last two represented by encompassing associations. All three partners are involved in the making and implementation of policy; institutions such as parliament play a comparatively minor role, as it is the executive that represents government. The essence of the corporatist bargain is that business supports policies geared to redistribution and full employment and does not disinvest in response to these policies. Labor, for its part, promises not to make life difficult for business through strikes or other forms of militancy. Government fosters the organization and representation of business and labor, guarantees their exclusive participation in policy-making, and in turn expects both business and labor associations to discipline their members (for further details, see Schmitter and Lehmbruch 1979). Corporatism of this sort has been practiced most effectively in Scandinavia, Germany, the Netherlands, and Austria, though elements of it exist in other European countries (and in Australia).

The democrat's problem with corporatism has always been that it is exclusive: Only business and labor are represented, and even the grass roots of labor have little influence. Interests defined on a noneconomic basis are shut out completely. Thus, recent theoretical works in the corporatist tradition (e.g., Schmitter 1992) have addressed how corporatism might be made more inclusive by granting a place at the table to noneconomic groups, such as environmentalists. The latter might agree to cease sponsoring boycotts, protests, or legal action against polluters and despoilers in return for commitments on the part of government, industry, and labor to antipollution measures and wilderness protection. Real-world corporatist systems have shown some signs of extending themselves in this direction. Notably, in Norway moderate environmental groups (such as Friends of the Earth) are partially funded by the state and have a recognized place in corporatist policymaking. Such extension—though for the most part only to interests defined on an economic basis—is exactly what Cohen and Rogers desire.

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[6] Early twentieth century pluralists such as Harold Laski (1919) and Mary Parker Follett (1918) have more in common with contemporary difference democrats than do Truman et al.

[7] Here, Walzer echoes the long-standing fear of mass society theorists such as Kornhauser (1959).

[8] A somewhat different model with the same name is developed by Hirst (1994). But whereas Cohen and Rogers propose an actively inclusive state, the state in Hirst's associative democracy is passively inclusive. In Hirst's model, democracy is built from the ground up by citizen associations which then take on many of the functions now performed by the state. The state's role is restricted to enabling such a process to occur, rather than to actively promoting it.

Iris Young applauds this kind of state-sponsored extension of representation, though she wants it to apply to the whole range of oppressed groups, not just economically defined ones. Connecting her earlier work on group representation with the idea of associative democracy, she avers in her commentary on Cohen and Rogers that "the state could decide to promote the self-organization of members of oppressed groups where such organization is weak, or to provide greater resources to existing associations representing oppressed or disadvantaged groups, and to create compensatory political forms to ensure that such groups have an equal voice in agenda setting and policy formation" (Young 1992, 532).

Theorists who seek democratization in the shadow of corporatism believe that corporatism's best quality is its ability to actively include particular interests; the problem is only that historically a very limited range of interests has been included. I will suggest, in contrast, that the real beauty of corporatism is in its passive exclusion of many interests in society—and that it does this with a state that seems quite good at promoting economic justice (at least in comparison to all the alternative forms of state organization that have been tried from time to time).

Actively inclusive states exist mostly in the proposals of political theorists such as Cohen and Rogers, Walzer, and Young. Yet, there is one real-world example, though not one these theorists are likely to endorse. The Mexican state's longevity and stability can be attributed to its brilliantly successful incorporation of successive waves of potential troublemakers. Until recently, both the means (patronage and coercion) and the ends were authoritarian. The 1988–94 Salinas administration's PRONASOL (National Solidarity Campaign), however, added the trappings of grassroots participatory democracy to centralized state guidance. PRONASOL was directed at the social movement activists involved with the Cardenista opposition, which had probably won the 1988 presidential election. (The government claimed otherwise.) Oriented to the organization of peasants, workers, and disadvantaged communities, PRONASOL dismantled the opposition and employed many of its activists (see Carruthers 1995). Thus, the Mexican tradition of a strong state dominating a weak civil society could continue, though now with an inclusive democratic aspect, or at least veneer.

Let me now try to add an historical dimension to the analysis, which will justify suspicion of the actively inclusive state and thus demonstrate why the Mexican outcome might well follow from the proposals of actively inclusive democratic theorists, however unwelcome this outcome might be to them. I will then outline the consequences of this suspicion for any strategy of democratization and proceed to consider where and how democrats might do better.

## INCLUSION IN HISTORICAL PERSPECTIVE

A democratizing strategy of the progressive inclusion of as many interests as possible in the state implies that the content of public policy is essentially indeterminate. Midcentury pluralists were most explicit on this point because they saw public policy, through a physics analogy, as the result of the direction and strength of whatever pressures were applied by different interest groups. Yet, this picture of indeterminacy is manifestly false, as pluralists such as Dahl (1989) and Lindblom (1982) themselves eventually came to recognize. Irrespective of what interest groups seek, states must meet certain imperatives. Unfortunately for advocates of state-sponsored group representation such as Cohen and Rogers and Young, promoting the organization of disadvantaged groups is not one of these imperatives, and I can imagine no scenario under which it becomes one. If so, then to advocate such representation may work as moral philosophy but not as political theory, for the latter must attend to practical constraints, as even Rawls (1987, 24) recognizes. Luckily for democrats, however, the imperatives of states are not constant over time; and this very inconstancy enabled democratization of the state to take place in the past. The implication here is that democratization of the state in the future is possible only to the extent that state imperatives continue to change. Before exploring this last point in more depth, let me turn to the content and history of the imperatives facing states.

According to Skocpol (1979), all states must survive in a hostile world, keep order internally, and extract the resources to finance survival and order, through some combination of taxation and compulsion of their own populations. When states alienate their own upper classes by leaning on them too heavily to finance their defense against external threats, these classes may withdraw support from the state, allowing revolution to commence. Skocpol applies this analysis to revolution in the agrarian bureaucracies of France, Russia, and China.

Though Skocpol writes as though these three imperatives apply unchangingly to all states, in fact their severe conjunction applies only in a Hobbesian world in which violent international conflict is a normal feature and the economic resource base available to states is more or less fixed. Conditions have changed for states now fortunate enough to belong to the global core, and they have been able to meet their imperatives in ways that have had positive consequences for democracy. (Life for states on the global periphery remains more Hobbesian.)

The first such modification comes with the rise of capitalist economies, and with them the potential for economic growth on a scale never seen before. States need no longer rely on punitive taxation or confiscation to finance their defense against external threats; economic growth can help perform the same function in a less painful fashion. Thus, the first imperative facing states in capitalist systems is what post-Marxists (e.g., O'Connor 1984, Offe 1984) call "accumulation." States simply must provide the conditions that facilitate capitalist investment and economic growth; if they pursue antibusiness policies, then they are punished by "capital strike," recession, falling tax revenues, and unpopularity in the eyes of the public. Block (1977) and Lindblom (1982) have detailed the degree to which this "exchange-dependency" constricts the policies of states; markets, according to Lindblom, "imprison" government policy, and "pluralism at most operates only in an unimprisoned zone of policy making" (Lindblom 1982, 335). It is markets, especially financial ones, not public opinion or parliament, that are the sounding boards for public policy. And if policies are constricted, then so is democracy, for policies that contradict the fundamental interests of business must be vetoed, no matter how popular. As Bowles and Gintis (1986, 90) put it: "The presumed sovereignty of the democratic citizenry fails in the presence of capital strike." The ever-increasing mobility of capital across national boundaries intensifies this constraint (Dryzek 1996, 77–83).

Today, exchange dependency figures mostly as an impediment to democratization, but the opposite was once true. The importance of the accumulation imperative in the development of democracy is that it brought the interests of the state and of the emerging bourgeoisie into harmony, so that participation in policymaking could be extended to the bourgeoisie. It is easy to forget that the bourgeoisie was once an oppositional force in Western societies and constituted a democratic civil society hostile to the state. Habermas (1989) details the democratic life of the early bourgeois public sphere, as manifested in newspapers, coffee houses, and public association. He also notes the subsequent migration of the bourgeoisie from civil society to the state.

This migration was followed somewhat later (the timing, of course, varying by country) by the migration of the industrial working class; and again, this development can be related to the shifting content of state imperatives. Capitalism produces an industrial working class; and, as Karl Marx recognized, the conditions of its existence in urban centers and large factories enable it to constitute a political threat to the state in capitalist society. At the time Marx was writing, the activists of the organized working class constituted an oppositional sphere that confronted the state. Marx himself thought that opposition would culminate in revolution. What happened instead was that opposition gave way to inclusion. That choice was far from easy for socialist parties to make, and syndicalists, anarchists, and revolutionary Marxists mounted a protracted resistance to it in the early twentieth century (see Przeworski and Sprague 1986, 13–28). But for better or for worse, workers' parties and trade unions did come to play significant roles in electoral politics and public policy-making in many industrial societies. The degree of their inclusion remains quite variable, however, reaching a high point in social democratic corporatist societies, and this inclusion has, for the most part, proven less easy and less complete than the earlier inclusion of the bourgeoisie. Strikes and demonstrations have been, at least until the last decade or so, part of the political repertoire of the organized working class.

Industrialization meant that the long-established state imperative of keeping internal order was no longer a matter of having coercive power sufficient to keep potential malcontents in line. Far more efficient, if it can be secured, is the *voluntary* acquiescence of potentially rebellious subordinate social classes and categories to the dominant political-economic order. Thus, the imperative of keeping internal order mutates over time into what neo-Marxists or post-Marxists such as Habermas (1975), O'Connor (1973), and Offe (1984), call legitimation. And the most effective device for legitimation so far devised, these authors aver, is the welfare state. In part, the working class can be induced to accept the capitalist political economy if it delivers the goods in the form of material prosperity, in which case legitimation can be assimilated to accumulation (assuming enough wealth trickles down). But the business cycles and employment insecurity endemic even to growing capitalist systems mean that something more is required, and this is where the Keynesian welfare state comes in. The fact that the Keynesian welfare state is now on the way out, or at least under attack, has some major negative consequences for democratization, which I will address shortly.

Legitimation is secured when subordinate classes and categories with the capacity to destabilize the political economy instead support or accept that structure. The best example of such a class or category in industrial societies has often been the working class, though groups defined on an ethnic or religious basis may also possess the capacity to destabilize. The threat to political stability posed by permanent ethnic or religious minorities forms the core of Arend Lijphart's argument for a consociational state that recognizes and incorporates the organizations of such minorities (see, e.g., Lijphart 1977). It should be stressed that the legitimation imperative does not reduce to a need for governments to court popularity. If it did, then every interest held by every group in society could be assimilated to the legitimation imperative, and the whole concept of a state imperative would dissolve into pluralism, thus losing all force. Only when the class or category in question has the capacity to destabilize is legitimation at issue. I will discuss the emergence of another threat to legitimation shortly.

## EMERGING STATE IMPERATIVES AND THE PROSPECTS FOR INCLUSION

The moral of the preceding historical tale is that oppositional groupings can only be included in the state in benign fashion when the defining interest of the grouping can be related quite directly to a state imperative.[9] Under this condition, groups can help determine the content of public policy, at a minimum influencing how imperatives are met (e.g., legitimation might be promoted by either universalistic or means-tested welfare state programs), and how trade-offs between competing imperatives are made (i.e., to what extent should economic growth be sacrificed for the sake of redistribution?).

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[9] It is conceivable that a group's interest has no bearing at all upon state imperatives, in which case its inclusion or exclusion is of little interest from the perspective of democratization, as by definition that group will have no concern with the key public debates of the day.

If the interest of an oppositional group cannot be so related to an imperative, then inclusion means that the group will be co-opted or bought off cheaply, as were the leaders of environmental interest groups who secured not only access but also employment at high levels in the Clinton administration, but found themselves unable to achieve much in the way of substantive policy. As Jay Hair, leader of the National Wildlife Federation, the largest of the co-opted groups, eventually put it, "What started out like a love affair turned out to be date rape" (Dowie 1995, 177). The group in question receives symbolic rewards only. Goodin (1980, 123–56) argues that such rewards are defensible if the symbols in question (e.g., religious rituals, flags, a member of one's ethnic group occupying the mayor's chair) have intrinsic value to the group. Symbolic rewards are correspondingly indefensible if they are offered as promissory notes for more tangible goods, but turn out to be substitutes for these goods.

To the extent that public policy remains under the sway of state imperatives, groups whose inclusion coincides with no imperative will not easily acquire the tangible goods they value. They may be allowed to participate in the policymaking process, but outcomes will be systematically skewed against them. Anything more would introduce a dangerous degree of indeterminacy into the content of public policy. A high price will be paid by any group included on this basis. For if state officials have no compelling reason to include the group, then presumably it must moderate its stance to fit with established state imperatives. Moreover, in entering the state, the group becomes constrained in the kinds of interactions in which it engages. In particular, the group may have to develop a more hierarchical internal structure in order to produce a stable leadership which government officials will recognize and deal with. Inclusion in the life of the state is, then, bought at the expense of relatively unrestricted democratic interplay in the oppositional public sphere. The democratic loss experienced by entry into the state can, as in the cases of the bourgeoisie and the working class in the past, be justified by the instrumental benefits thereby achieved. But if there are few or no instrumental benefits, the loss is harder to justify.

Of the group claims now endorsed by difference democrats and other advocates of inclusion, which, if any, can be related to established or emerging state imperatives? Their experience in the Clinton White House notwithstanding, I would consider the best claimants to be environmentalists, and the relevant emerging state imperative to be environmental conservation. This imperative itself develops out of the accumulation and legitimation imperatives. Now, accumulation and environmental conservation have traditionally been cast in a zero-sum relationship: Economic growth has to be foregone if the people are to enjoy environmental values. But recent thinking reconceptualizes this relationship. Albert Weale (1992), among others, argues that the essence of the new environmental politics is "ecological modernization": the idea that economic growth and environmental values now stand in a positive-sum relationship because a clean environment is good for business. Why? First, a pleasant environment can substitute for income for employees. Second, dirty air and water hurt productivity. Third, consumers increasingly demand environmentally benign goods and services. Finally, pollution indicates inefficiency in materials use. Weale argues that ecological modernization has been incorporated into government policy most effectively in Germany; Hajer (1995) compares its limited progress in Britain with its more substantial gains in the Netherlands. In the United States, the kind of green capitalism advocated by Vice President Albert Gore (1992) is consistent with ecological modernization, though he does not use the term.

Environmental conservation can be linked to the legitimation imperative via Ulrich Beck's (1992) notion of "risk society." To Beck, politics is increasingly organized around the production and distribution of risks (mostly related to chemical, radioactive, and biotechnological hazards) rather than material goods. Those exposed to risks are so numerous, and so capable of political mobilization, that they threaten the stability of the political-economic order, and thus place legitimation at issue. In the face of widespread public mistrust of arrogant scientists and technologists and of their corporate and governmental employers, Beck believes that legitimation in risk society can only be achieved by public participation in risk selection, allocation, and amelioration.

Even if the ecological modernizers and risk democrats are correct, not all environmental concerns can be easily assimilated to state imperatives. Endangered species protection, wilderness preservation (especially when the wilderness contains valuable extractive resources), animal rights, and deep ecological conceptions of how to live in relation to nature are some of the aspects of environmentalism not easily assimilated. The solution here might be a "velvet divorce," in which part of the environmental movement enters the state, and part chooses to continue to confront the state from the public sphere. This describes the situation in a number of countries. Until their demise in the early 1990s, Green Fundis in Germany maintained a confrontational stance while their Realo counterparts pursued an ever more conventional electoral strategy. In the United States, a clear distinction remains between mainstream environmental groups on the one hand and radicals, such as Earth First!, animal liberationists, and networks organized around toxics and environmental justice issues on the other.

One might analyse contemporary feminism in similar terms: Liberal feminism is destined for the state, whereas cultural feminism is more suited to the public sphere. The feminization of work means that women's concerns about employment can be related to the accumulation imperative, as the prohibition of discrimination against women can actually be good for business in general. If the patriarchal family is a remnant feudal structure, as Fraad, Resnick, and Wolff (1994) argue, then capitalism is now destroying feudalism's last holdout by bringing women into the paid labor force (see also Beck 1992, 105). The feminization of poverty might indicate that aspects of feminism could be assimilated to the legitimation imperative, given the latter's association with the welfare state. But because poverty-stricken households headed by females do not constitute much of a political threat to the established order (in comparison with the organized working class during the period of its greatest strength), this assimilation seems unlikely. Cultural feminist demands for a different kind of politics based on the ethics of care and nurturing have no obvious connection to any state imperative, and so should not expect advances within the state.

## TO CIVIL SOCIETY

As yet I have said little about the precise characteristics of civil society and the public sphere as alternative sites for the pursuit of democracy. Civil society is an inexact concept, used in different ways by different authors. As a first approximation, civil society may be defined as all social interaction not encompassed by the state or the economy. In its political aspects it also excludes private life, although recent attacks by feminists and others on the public/private distinction make this boundary less clear. When discussing the prospects for democracy, the politicized aspects of civil society are most interesting. In this political sense, civil society consists of voluntary political association oriented by a relationship to the state, but not seeking any share in state power; that is, association is self-limiting (see Isaac 1993).

Prominent examples of civil society in action would include the early bourgeois public sphere discussed by Habermas (1989), the insurgent "free spaces" in U.S. political history constituted by women, blacks, workers, farmers, and others (Evans and Boyte 1986), the democratic opposition in Eastern Europe prior to 1989 (Arato 1993), and, in the West, feminist, antinuclear, peace, environmental, and urban new social movements. Such public spheres often feature relatively egalitarian and discursive politics in their internal workings (for further details, see Dryzek 1996, 47–53). They do not pursue power as interest groups or through electorally oriented parties; yet, they are, of course, concerned with public affairs. Often this concern casts them in opposition to the state and prominent economic actors, although sometimes state and corporate power can be ignored.

Civil society is a heterogeneous place, home to the Michigan Militia as well as the movements I have mentioned. Other groups—for example, Greenpeace—may be less overtly hostile to democratic values, but still quite hierarchical. Not everything in oppositional civil society represents discursive democratic vitality. So how do we distinguish between civil society formations that contribute to democratization and those that do not? Offe (1985, 853) excludes the religious right in the United States and neofascists in Europe from his purview by applying the criterion of commitment to "a selective radicalization of 'modern' values." Thus, to Offe social movements are relevant to democratization to the degree that they fit an account of progress toward fulfilment of modernity's potential. Offe's viewpoint dovetails quite easily with my analysis of evolving state imperatives and their relation to changing patterns of inclusion and protest. While it is rare in practice, however, oppositional civil society groups can be both conservative in aims and democratic in internal structure. The U.S. anti-abortion group Operation Rescue might fall into this category. Its conservatism alone does not warrant its dismissal. Nor is hierarchy alone sufficient reason for dismissal: Despite its internal hierarchy, Greenpeace contributes to the democratic interplay of oppositional civil society. It is less easy to see how groups that are both conservative and hierarchical make any such contribution.

Self-limitation does not mean that civil society is a powerless realm. Power can be exercised from, and within, civil society in several ways. First, political action can change the terms of political discourse and so affect the content of public policy. The rhetorical achievements of Martin Luther King are exemplary here. King drew upon and reshaped the discourse of American constitutional liberalism in order to advance an agenda of civil rights for African-Americans. The women's movement has succeeded (not without resistance) in changing the ways in which gender, family, and the dividing line between public and private affairs are conceptualized in policy debates. In such fashion can "communicative power" assert itself over "administrative power," as Habermas (1992) puts it. Communicative power is diffuse and pervasive, felt in the way terms are defined and issues are framed, not in the direct leverage of one actor over another.

Second, as Tarrow (1994, 184–6) argues in reply to those who believe waves of political protest leave behind only burned-out or co-opted activists, social movements can produce lasting effects in political culture by legitimating particular forms of collective action, such as the sit-in, and by establishing a permanent place for issues on the public agenda.

Third, policy-oriented fora can be constituted within civil society. A good example is the Global Forum, which assembled as the civil society counterpart to the 1992 United Nations Conference on Environment and Development in Rio. Composed of nongovernmental activists from all over the world, the Global Forum influenced what occurred in the official proceedings of the conference, in part by shaming and embarrassing some of the official participants. Domestic environmental policy is sometimes influenced by such fora, concerned with issues such as renewable resources (Berger 1985) and toxic wastes (Fischer 1993).

Fourth, protest in civil society can create fear of political instability and thereby draw forth a governmental response. Piven and Cloward (1971) interpret the history of the U.S. welfare state in these terms: Welfare provision increases only in response to unruliness on the part of the poverty-stricken. Piven and Cloward do not imply that protest should be confined to civil society; they advise the poor to step up their demands on the state through conventional channels. Yet, their historical account can be deployed as evidence of the power of civil society over the state.

These four civil society activities involve the more or less democratic exercise of power *over* the state, but they do not reduce to the inclusion of civil society *in* the state. In addition, civil society can reclaim power *from* the state—and from the economy. Indeed, Janicke (1994) defines civil society in functional terms, as public action in response to failure in government and the economy. This functional definition highlights the idea that civil society can itself feature problem solving, not merely talk. Civil society can, then, engage in "paragovernmental" activity, not simply act as a source of influence over the more obviously governmental activities of the state. There are numerous examples of this kind of activity. When feminists and others speak of "empowerment," they do not mean influence over government but rather control of their own lives, facilitated by support groups and the like. Disputes within communities can be settled through alternative dispute resolution without involving courts, as for example in the community boards of San Francisco (Schlosberg 1995). Citizens can exercise power directly over economic actors through means such as boycotts of corporations or products. In 1995 Greenpeace organized protests against the Shell Oil corporation's plan to dispose of the redundant Brent Spar oil platform in the North Atlantic. Shell eventually capitulated, much to the annoyance of the British government, which was prepared to use force to dislodge protestors from the platform.

A final and somewhat different way in which power can be exercised through civil society is by means of cultural change that affects power relations. Think, for example, of the extent to which feminism has changed power relationships both within and outside the family (and not just as a consequence of changes in family law). Even if civil society actions leave public policy untouched, they can have real social effects. Along these lines, Tesh (1993) sees the success of new social movements in terms of the changes they produce in ethics and culture, and therefore in behavior in everyday life, for example in relationships between the sexes, or in people's awareness of pollutants and the environmental friendliness of products.

Civil society can constitute a site for democratization because it can be a place where people choose to live their public lives and solve their joint problems. Those who see deliberation as the essence of democracy (e.g., Cohen 1989, Miller 1992) should be attracted by the discursive style of public spheres. The postmodern theorists (e.g., Connolly 1991), who are very different in that they conceptualize democracy in terms of agonistic respect established through the negotiation of identities and differences, should likewise be attracted to the pluralistic politics of identity in the public sphere. Yet another form of democracy, Hannah Arendt's democratic "oases in the desert" of a sterile modernity, can only be found in civil society. Critics of Arendt argue that she was happy to purchase democratic authenticity at the expense of democratic franchise. But as Isaac (1994, 158) points out, the "elites" populating these oases are self-selected and can come from any social class; and they rule over themselves, not over anyone else, which is why Arendt viewed a workers' council as an exemplary "elite."

But why should civil society often be more attractive than the state as a site for democratization? The answer is that it is relatively unconstrained. Discourse need not be suppressed in the interests of strategic advantage; goals and interests need not be compromised or subordinated to the pursuit of office or access; embarrassing troublemakers need not be repressed; the indeterminacy of outcome inherent in democracy need not be subordinated to state policy.

## THE DEMOCRATIC BENEFITS OF EXCLUSIVE STATES

There is, then, much to be said for democratic life in civil society as opposed to democratic life in the state. But the state and its structure cannot be ignored, for how states are organized turns out to have major implications for the democratic vitality of civil society. If the impetus for democratization begins in oppositional civil society rather than in the state—and I would suggest that this has almost always been true historically—then, counterintuitively, a degree of *exclusion* in the pattern of state interest representation is desirable if civil society and so democracy itself are to flourish. But what kind of exclusionary state is at issue here?

I have already noted that inclusive states can be either passive (in the sense of accepting whatever groups emerge from society) or active (in taking steps to mobilize particular groups and shepherding them into the state)—and that both passively and actively inclusive states have their hazards for democracy in civil society. Similarly, states that pursue exclusion can do so in either an active or a passive fashion. Active exclusion implies a state that attacks and undermines the conditions for public association in civil society. Passive exclusion implies a state that simply leaves civil society alone.

Examples of these two kinds of exclusion can be found in the histories of both the West and the former Soviet bloc. In the Soviet bloc, active exclusion characterized true Stalinism, under which any sign of political organization separate from the state was sought out and snuffed out. With time and the loss of true believers, the Stalinist state's energies flagged in this respect. The more passive exclusion of halfhearted Stalinism, especially in Poland, Hungary, and Czechoslovakia, proved far more conducive to the establishment and survival of oppositional public spheres (see Bunce 1992). Movements such as Solidarity in Poland and Charter 77 in Czechoslovakia had no access to the state, but neither were they obliterated (even though their members were harassed and occasionally imprisoned).

In the West, the main actively exclusionary form of state interest representation is authoritarian liberalism, practiced with varying zeal in the Anglo-American world in the 1980s (and, in some cases, in the 1990s). Authoritarian liberalism involves maximization of the role of the market in organizing society, in combination with attacks on the conditions of public association in civil society. Along these lines, Britain under Margaret

Thatcher experienced attacks on the ability of trades unions to organize workers, on press freedom, on the ability of civil servants to divulge information (even about government activities unrelated to national security), on the legal rights of defendants in court cases, on the freedom of local authorities from central government control, on the independence of the British Broadcasting Corporation, and on the political neutrality of the police. Not only did the collectivism of the left come under attack but also organic "one nation" Toryism was destroyed by the Thatcherites. Authoritarian liberals even tried, with some success, to reverse the historical inclusion of the working class in the state. The Trades Unions Congress was expelled from the quasi-corporatist National Economic Development Council, and unions were subjected to new legal restrictions on their ability to organize workers and take industrial action. The inclusion of the working class had never been very pronounced in the United States, but the expulsion of organized labor was symbolized by the Reagan administration's unopposed destruction of PATCO, the air traffic controllers' union, in 1982. In both cases, the legitimation or welfare state imperative associated with the initial inclusion of the working class also came under attack, and more coercive means of social control came back into fashion. All this can be attributed, in part, to deindustrialization and the associated dissolution of the working class. The organized working class became less of a threat than it once was, and so its link to the legitimation imperative could be weakened, if not severed.

These kinds of governmental attacks on the conditions for association in civil society can be hindered, if not prevented, by constitutional restraints. Walzer (1991) argues that the Bill of Rights in the United States Constitution protects not only the private realm of individual life but also the associational realm of civil society. According to Walzer's account, the authors of the Bill of Rights assumed the existence of this associational realm, whose subsequent atrophy meant that these rights came to be interpreted as applying to private individuals rather than to public associations. Still, the absence of any comparable constitutional defenses in Britain may help to explain the relative ease with which the Thatcherite agenda for the individualization of civil society was implemented in the 1980s. Earlier I argued that one should not expect a great deal in the way of positive commitment to the associational life of civil society on the part of governmental officials, the hopes of Walzer and others notwithstanding. Constitutional and legal defenses for civil society should be welcomed, however, when they can be obtained from governments. Beyond the Bill of Rights, other U.S. examples are the 1935 National Labor Relations Act, which established the rights of unions to engage in collective bargaining, civil rights legislation in the 1960s, and more recent changes in family law. Such measures need not be accompanied by the entry of the protected associations into the state in the terms I defined at the outset.

Such constitutional and legal restraints are less necessary under corporatism. Corporatism, as I noted earlier, is characterized by a bargain involving government, business, and labor, the terms of which do not allow other interests to have any say in policy formation and implementation. Corporatist states do not attack or undermine the conditions for public association in civil society; they simply ignore it by offering no channels of access to the state. Thus, the passive exclusion associated with corporatism is more benign for democracy in civil society than is the active exclusion of authoritarian liberalism. One major reason for the rise of new social movements representing environmental, feminist, antinuclear, and peace values in Europe in the 1970s and 1980s was the exclusive pattern of interest representation adopted by corporatist states such as West Germany, the Netherlands, and Austria (Kitschelt 1988). If one also notes here the comparative effectiveness of these states in delivering social justice (via income redistribution) full employment, and even economic growth (Freeman 1989; Pekkarin, Pohjola, and Rowthorn 1992), then there is indeed much to be said for social democratic corporatism as a form of state organization. The democratic potential of corporatism is not in the inclusions it can arrange, but in the exclusions it generates by default.

An examination of contemporary states using comparative statics would conclude that corporatism is the state model most conducive to a democratic civil society (see also, Dryzek 1996, 64–70). But an historical extension of the analysis reveals that corporatist states are themselves the product of inclusion in the state of groups previously operating in civil society, first the bourgeoisie and then the organized working class. These two inclusions could proceed only under particular conjunctions of group interest and changing state imperatives. As I have indicated in discussions of environmentalism and the women's movement, changing circumstances might lead to a revision of this provisional verdict on corporatism, especially if the latter is defined in strictly tripartite terms. The provisional conclusion in favor of passive exclusion, of which tripartite corporatism is the most visible contemporary example, is less easily shaken.

For better or worse, corporatist states may continue to change in response to groups operating in civil society and claiming access to the state (or indeed forsaking the state in favor of civil society). Such claims are sometimes futile if the state has no obvious point of access. But even strong corporatist states, which Lehmbruch (1984) identifies as Austria, the Netherlands, Norway, and Sweden, sometimes open up access points—for example, for environmentalists in Norway in the 1990s, as I noted earlier. And most contemporary states in developed societies are not strongly corporatist, instead falling somewhere on a continuum between corporatism and pluralism. (See Lehmbruch 1984 for a classification of countries in these terms.) What, then, should guide the strategic choices of groups when the possibility of access to the state does arise?

## MOVEMENT STRATEGY AND STATE RESPONSE

Observers of social movement dynamics often argue that eventually groups must choose the state over civil society. Given that mass activism is hard to sustain, what Tarrow (1994) calls "cycles of protest" normally end in some degree of bureaucratization coupled with incorporation into the state, at least in liberal democratic societies. Indeed, the movement may not escape fading into oblivion, and in more authoritarian systems, it may end in repression. To the extent that such cycles are inescapable, entry into the state (through any, some, or all of the means I defined at the outset) may be a pragmatic necessity, not a free strategic choice. Claus Offe (1990) argues that movements normally pass through three stages. The "takeoff" phase in civil society is informal, spontaneous, and militant. Such inchoate, unfocused energy fades as the movement reaches what he calls a "stagnation" phase ("consolidation" would have fewer pejorative connotations), involving definition of group membership, leadership, and organization. The third stage is "institutionalization," or what I have termed inclusion in, or entry into, the state. Offe believes that such institutionalization can be expected as a rational use of limited resources of time, energy, and finance and as a way of sustaining a movement when supporters are not ready to contribute anything more than votes and money. The movement can then "cash in" the resources it has mobilized in its takeoff and consolidation phases to achieve access to real political power (p. 243). Thus, Offe believes that entry into the state can be a good bargain. Other observers (for example, Lowi 1971) believe that such entry normally means co-option. I would suggest, in contrast, that one cannot generalize about and evaluate such entry in any sweeping terms. Whether the third stage of the life cycle should be welcomed or lamented depends crucially on the particular configuration of movement interests and state imperatives.

A close look at the historical record casts doubt on this life-cycle interpretation of social movements. Offe himself admits that when the available range of public policy solutions to movement concerns is manifestly inadequate, then there are good reasons for a section of the movement to resist institutionalization—an insight he applies to the German Greens (Offe 1990, 246–7). Rucht (1990) notes that new social movements, in particular environmentalism in France and Germany, have featured simultaneous and sustained action in the state and civil society. Cohen and Arato (1992, 555–7) make a similar observation about feminism in the United States. This record suggests that groups are not locked into any simple life cycle; they do have choices. Faced with such a choice, should a group choose civil society, the state, or both simultaneously?

Cohen and Arato (1992) advance a blanket "both" guideline, which they call a "dualistic" strategy for social movements. They regard the women's movement as exemplary: "The dual logic of feminist politics ... involves a communicative, discursive politics of identity and influence that targets civil and political society and an organized, strategically rational politics of inclusion and reform that is aimed at political and economic institutions" (p. 550). In civil society, movements would act "to redefine identities, to reinterpret norms, and to develop egalitarian, democratic associational forms" (p. 531). In the state, groups would not only pursue group goals, but also seek the development of a supportive constitutional, legal, and policy context for continued movement activity in civil society. Legislation and policy in turn would draw sustenance from a supportive cultural context in civil society (p. 552). A large part of the justification for their dualistic strategy, therefore, turns out to be the degree to which the state can influence the democratic condition of civil society, for better or for worse.

A similar "both" position is argued by Hilary Wainwright (1994), though for the sake of the achievement of substantive group ends rather than for the democratic vitality of civil society. She avers that movement goals related to, for example, ecological and equity values "require democratic decision making with binding national and international authority" which can only be supplied by the state (p. 195). Without movement activism in civil society, such public policy action is unlikely (p. 197); but again, echoing Cohen and Arato, "nonstate forms of political action need a supportive and independent relationship to political power if they are to be effective agents of economic and social change" (p. 190). Though sympathetic to Offe's life-cycle reasons for entry into the state, she believes that parliamentary activity can involve permanent sustenance for the extraparliamentary movement, rather than constituting a permanent substitute for the latter (p. 196).

These arguments for a "both" answer or a "dualistic" strategy appear to be good ones. But they imply a benign view of the actual or potential motivations of government officials. One might hope that public officials would recognize the need for a lively civil society and formulate policies to promote it, perhaps even along the lines proposed by Walzer that I discussed earlier. The problem remains that there is absolutely no reason for public officials to behave in the way Walzer suggests, and every reason for them to behave otherwise if the strengthening of particular civil society groups clashes with an established state imperative. Walzer's own agenda of stronger unions, housing cooperatives, workers' cooperatives, and subsidized community-based welfare provision promises plenty of trouble for state officials.

Beyond laws protecting the basic citizenship rights of expression and association, one should not expect much in the way of positive state action to promote the well-being of civil society. Cohen and Arato and Wainwright might object here that, unlike Walzer, they do not seek much in the way of positive commitment by state actors, they merely advocate acceptance of the legitimacy of movement goals and of the continued linkage of group leaders with more uncompromising extraparliamentary movement wings. But such tolerance may be stretched to the limit if movement goals, especially as articulated in radical terms by the civil society wing, clash with state imperatives. In this light, let me suggest that two criteria are relevant to any group's choice between civil society, the state, and a "dualistic" strategy. First, the group should consider whether its defining interest can be assimilated to any state imperative. If the answer is no, then entry into the state is a poor strategy in instrumental terms, for it is unlikely that the group's goals will be embodied in public policy; and entry is bad for democracy, because the vigorous democratic life of the public sphere will be forsaken in favor of co-option and a politics of symbolic rewards.

This first criterion may not always allow a once-and-for-all answer. I noted earlier that state imperatives change with time. Such changes are not easily predicted, but if they do occur, a group should be prepared to change its choice. Around 1980 it would have been hard to predict the assimilation of environmental concerns to the imperatives of accumulation and legitimation. Developing notions of ecological modernization and risk society have made this assimilation possible, as I noted earlier. In Germany, these developments help to justify the "Realo" Green Party's eventual choice of wholehearted entry into the state. (One might argue that these developments are themselves due, in part, to a social learning process initiated by the Greens; but such learning can issue from civil society just as easily as from, or within, the state, and so provides no argument for preemptive entry into the state.)

Changing state imperatives might also lead a group to reconsider a past decision to enter the state. Such entry may have been a good bargain for the organized working class for most of the twentieth century. More recently, deindustrialization and the consequent decline in the numbers of industrial workers and limitation of the scope of the Keynesian welfare state mean that working-class interests are no longer as easily aligned with the legitimation imperative as they once were. These developments and difficulties, however, have not persuaded the leadership of social democratic parties to contemplate a return to oppositional civil society. To the contrary, their normal response has been strategic moderation in the hope of appealing to non–working-class constituencies, or positioning the party to secure what Kitschelt (1994) calls the "pivotal" vote in coalition politics, having abandoned any desire to govern on behalf of an electoral majority garnered from the working class. But this strategic moderation has itself led some activists to abandon the party in favor of civil society alternatives.

The second criterion to be considered is whether the group's entry into the state would leave behind a flourishing civil society. If the answer is no, then a depleted civil society would mean a less democratic polity, even though it might mean a more democratic state. But even the latter is unlikely. For if all disadvantaged and oppositional groups commit themselves to conventional political channels, then there is less reason for the state to include them. In this context, Fisk (1989, 178–9) argues that "only if there is a continuation of politics by extraparliamentary means will democracy be able to establish limits to the power of a dominant class," because extraparliamentary protest is a standing warning to this class of what might happen if it is unresponsive to demands made through conventional channels (see also Wainwright 1994, 197). And, of course, much is lost with the depletion of civil society. This loss is not always noted, still less lamented, but an example of such depletion can be seen in connection with the success of socialist parties in electoral politics. As Przeworski and Sprague (1986, 184) observe, with this apparent success these parties "demobilized those potential efforts—cooperatives, councils, and communes—that could not be channelled through elections; they deprived grassroots initiatives of a chance to experiment and grow autonomously; they turned nascent movements into compliance with electoral tactics."

Consider also, in light of this second criterion, the migration of East European civil society into the state during and following the successful revolutions of 1989. This migration left behind little or nothing in terms of oppositional public spheres. The gain was a liberal democratic state; the loss was of discursive democratic vitality. Former participants in and observers of oppositional civil society experienced a real sense of loss (see Ash 1990). Such losses can also be observed in connection with the entry of the Campaign for Nuclear Disarmament into the British Labour Party around 1960, the entry of the 1960s U.S. civil rights movement into the Democratic Party, the demise of the "Fundi" Greens in Germany in the 1990s, and the Mexican opposition's participation in the regime's PRONASOL initiative after 1988, which I discussed earlier.

Now, not all civil society groups care about democracy, whether in their internal workings or in the polity more generally. Some groups, therefore, might see no reason to apply these two criteria, if it is only the well being of democracy that is at issue. Both criteria, however, have an instrumental as well as a democratic aspect. The first criterion asks a group to consider whether its presence in the state will indeed be accompanied by real influence. The second asks the group to contemplate the potential impairment of its influence resulting from the loss of a standing warning to dominant actors attendant upon the group's wholesale commitment to conventional politics. Thus, the two criteria are relevant to group strategy irrespective of the degree of commitment to democracy as such.

Whether a group should choose the state, civil society, or both simultaneously depends on the particular configuration of movement interests and state imperatives. None of these three answers is right for all movements at all times and in all places. I would argue that the popular "dualistic" strategy is appropriate only when some but not all of a movement's defining interests can be assimilated to state imperatives. As I argued earlier, liberal feminism may be at home in the state, whereas cultural feminism should remain in civil society. Similarly, environmental concerns that can be connected to risk, ecological modernization, or sustainable development can be expressed in state-related action, while the dimensions of environmentalism that pose a more radical challenge to the imperatives of industrial society and its governments belong in civil society. Sometimes the two wings may choose to part company. For example, on the one hand, the emerging network of groups concerned with environmental justice and toxics issues in the United States have distanced themselves from mainstream groups to the point of refusing the label "environmentalist." On the other hand, mainstream groups may, overtly or covertly, welcome the activities of their more radical counterparts in civil society; note the variety of reactions mainstream environmentalists have to the radical Earth First! group in the United States. At any rate, in such "dualistic" circumstances the identity of the movement as a single entity should also be on its agenda. (For an account of these tensions in American environmentalism, see Dowie 1995.)

A group's calculus in terms of the two criteria will be influenced by the mode of inclusion available to it. No unrestricted choice of mode is at issue here, for the structures of different political systems provide different opportunities. For example, in the United States there are many barriers to, and few opportunities for, new political parties; therefore, a movement's entry to the state normally comes via interest group politics. Germany, in contrast, provides public subsidy and free media access for any party once it has gained a small percentage of the vote, but it allows relatively few opportunities for U.S.–style interest groups. Inclusion is therefore more likely via a party route. It is hard to generalize about the effects of these variations in structural opportunity, if indeed they do make a difference; a great deal depends on the details of particular cases. For example, the national list system of proportional representation in Israeli elections makes inclusion via the party route very easy, but at the same time mandates the specification in very precise detail of a national party hierarchy. Other proportional representation systems work on a constituency basis, and thus provide more scope for local party organizations to retain some autonomy. One might hypothesize that the latter would be more conducive to the continuing democratic vitality of civil society than the former, and so to a "dualistic" movement strategy; but this is just a hypothesis. Observers of social movement dynamics and life cycles rarely draw any distinctions between, or make comparisons of, the different ways a movement can ultimately be incorporated into the state, so there is little useful comparative literature to draw on here.

Life-cycle theorists and dualists alike hold that some degree of association with the state is eventually necessary and desirable, if only to maintain a powerful and permanent presence for a movement. But, as I argued earlier, choosing civil society rather than the state does not necessarily imply choosing powerlessness. Nor does this choice imply impermanence. A movement's concerns can persist even when its action is less visible, for civil society can be sustained in social and political networks and reproduced through cultural transformation after more spectacular manifestations of the movement have passed (see Melucci 1989, 206; Tarrow 1994, 176–7).

## CONCLUSION

Let me now summarize my account of the dynamics of democratization. First, democratization is largely, though not exclusively, a matter of the progressive recognition and inclusion of different groups in the political life of society. This general inclusion is in turn sometimes manifested in inclusion in the life of the state. Because pressures and movements for democratization almost always originate in civil society rather than in the state, however, a flourishing oppositional civil society is the key to further democratization. This sort of civil society is actually facilitated by a passively exclusive state. A truly inclusive state would corrode the vitality of civil society (as the Mexican case illustrates), and so undermine the conditions for further democratization. Thus, every historical step the state takes toward inclusion should produce a pattern of exclusions as well. These exclusions are the seeds for, if nothing else, future and further democratization of the state; and they offer protection against the state's reversing its democratic commitments. Exemplary here was the creation of the social democratic corporatist state: labor was explicitly included, and a whole range of other groups were implicitly excluded by the very way in which labor was included. Thus, democrats, even difference democrats, should not interpret democratization as a matter of the state recognizing and welcoming an increasingly diverse range of groups and interests. Inclusion of this sort is only benign when a group's defining interest can be associated with an established or emerging state imperative, and when entry into the state does not unduly deplete the civil society left behind. Occasionally, these criteria allow a group to operate in both civil society and the state; but often they dictate that one or the other be chosen.

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# Hume's Constitutionalism and the Identity of Constitutional Democracy

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*Modern constitutional democracy entails a particular kind of political self-understanding that uniquely centers on a constitution. While many recent studies have focused on how constitutional text shapes this self-understanding, little attention has been paid to the implications of different views of constitutional authority. This is a critical consideration, however, because constitutional authority has always been intrinsically fragile within constitutional democracy, and never more so than at present. In this article, I explore the potential of constitutional science to generate a conception of constitutional authority and collective identity. I focus on David Hume's effort to use constitutional science to shape opinion about liberty and the nature of the political community. This analysis also provides a basis for reflecting on the problematic relation of democracy to constitutionalism and on the peculiar problem of constitutional opinion in constitutional democracy.*

In addition to certain institutional arrangements that limit and enhance governance, modern constitutional democracy entails a certain mode of political self-understanding. Much of the recent study of constitutionalism has helped us better understand how debate over constitutional text shapes this self-understanding (Agresto 1984, Barber 1984, Fisher 1988, Harris 1993, Macedo 1990, Wolin 1989), but relatively little attention has been paid to the dynamics of constitutional authority. Yet, the special authority of a constitution within a particular community is not a given; its authority must be established in some way, and the basis upon which it is understood to rest will have definite implications for the community's self-understanding.

In constitutional democracies, constitutional authority is generally thought to derive from consent or from some principle of democratic or individual choice, but this simplistic conception does not do justice to the complex phenomenon of constitutionalism. A more sophisticated account roots constitutional authority in the inadequacy of pure democracy and the need to improve it by subordinating democratic choice to a superior or original law. A constitution is thus commonly understood to be both the means to better democracy and *above* democracy. A constitution is in this sense a "higher law" that protects private rights through textual and institutional restraints.[1] In reigning over democracy, however, constitutional authority is intrinsically fragile: It is not directly supported by the authority of God or some other superior being; practically, it stems largely from the mere fact of the constitution's age; and it is powerfully opposed by the tendencies of democracy itself (Tocqueville 1969, 2:429–33). Such fragility raises questions about the very soundness of the modern constitutional enterprise and has no doubt contributed to the weakening of the constitutional idea in our times (Berns 1993, 42–5; cf. Levinson 1988).

With this problem of constitutional authority in mind, I turn to the constitutionalism of David Hume. Hume's constitutionalism is useful because he affords a unique view of a source of authority that is implicit in the constitutional idea but is rarely given its due: the wisdom arrived at through a constitutional science that rationalizes the constitution and explains its goodness. Hume affords us this view because he seeks to found the constitutional order on a kind of constitutional *opinion* that recognizes the near-exclusive sovereignty of constitutional science.[2] In so doing, Hume makes constitutional science not only the practical means of political liberty but also the very means for understanding what political liberty is and how it defines the political community. In a word, Hume makes constitutional science the *prism* of constitutional identity, thereby departing from an unscientific traditional constitutionalism without embracing the popular foundations of the emerging order of constitutional democracy.

To be sure, constitutional science has always been a feature of constitutional democracy, and it has always played some role in shaping the collective identity of actual constitutional democracies (in America, for example, through study of *The Federalist Papers*). But since its origin—and especially in recent times—this identity has been largely determined by the principle of popular sovereignty (cf. Elkin 1993, 117), and the authority of modern constitutions has tended to be identified with this principle or the authority of an ancient law rather than that of constitutional science. Even contemporary defenders of constitutional authority tend to slight the contribution of constitutional science; their emphasis on the constitution's status as authoritative law leads them to understate its scientific reasonableness (cf. Bork 1990). Thus, to better understand the potential of constitutional science to shape constitutional identity, it is useful to return to an earlier model of a constitutionalism in which the authority of constitutional science is supreme. Accordingly, in what follows, I will examine Hume's efforts to base constitutional government on

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[1] According to McIlwain (1947, 115, 142) this particular understanding of "higher law" is the distinctively modern understanding of constitutionalism. For similar views, see Friedrich 1946, 4, 21; Kraynak 1987, 1177; Sartori 1962, 863; Wormuth 1949, 3.

[2] Among all the writers contributing to the vast body of literature on Hume's moral and political thought, only Stewart (1992) focuses explicitly on Hume's concern with opinion. Stewart links this concern to Hume's agenda for political reform but not to his political science per se. Hont (1994) and Gunn (1983, 264) locate Hume's concern with popular opinion within a long tradition of apprehension of democracy.

rational obedience and constitutional science, and then I will conclude with some reflections on the peculiar problem of constitutional opinion in constitutional democracy.

## THE POLITICS OF OPINION

The central place of popular opinion in modern constitutional democracies stems, in large part, from the Enlightenment intrusion of theoretical principles into politics. This intrusion supposes that every human being is capable of comprehending and guiding his or her life by fundamental principles of moral and political right. As Kant says, the motto of enlightenment is "have courage to use your own understanding!" (1983, 41). Hume thought the new prominence of theoretical principles in politics "the most extraordinary and unaccountable *phenomenon*, that has yet appeared in human affairs," and he wrote a well-known essay opposing the kind of politics it inspired (1987a, 465–87). Such opposition has been widely interpreted as part of Hume's suspicion of reason's capacity to guide political life and of reason more generally (Conniff 1976; Livingston 1984; Mill 1969, 10:80; Miller 1981; Whelan 1985). While helpful in some respects, this antirationalist reading of Hume's politics has been taken too far under the influence of the canonical interpretation of Hume's epistemology as promoting some form of antirationalism, moral subjectivism, emotivism or cultural relativism.[3] Accordingly, it is necessary to affirm, as John Stewart (1992) does in his recent work, that the prevalent "picture of Hume clinging timidly to a raft of custom and artifice because, poor skeptic, he had no alternative, is wrong" (p. 8). While Hume opposed the new theoretical consciousness of Enlightenment, he did not think the answer lay in an unquestioning deference to existing customs and institutions. Indeed, most troubling for the antirationalist reading of Hume's politics is its neglect of his efforts to improve politics through political science.

Nonetheless, Hume's opposition to the modern intrusion of theoretical principles into politics does set him apart from other Enlightenment thinkers: It leads him to a unique understanding of the sort of opinion that best supports a free society. This understanding stresses the mediating effect of prudence and constitutional science on theoretical rigidity and precision. I will explore this understanding in the sections that follow; in the remainder of this section, I will briefly develop Hume's critique of theoretical politics in order to better explain the concerns that led him to undertake this novel approach.

For Hume, the main problem of theoretical politics is not so much the content of modern theories or their effect on the mind, but rather their exploitation by designing elites. In his view, Christianity's separation of religious authority from political authority was the catalyst for theoretical politics because it liberated religious leaders from the control of their political counterparts and gave them a new means that they could employ to pursue their interests. Modern "factions of principle" are thus really "factions of interest," and speculative principles are tools of manipulation (1987a, 61–2). Accordingly, the practical problem of theoretical parties must be met with a practical or institutional solution: Religious authorities must be brought back under the wing of the state, and the "interested diligence" of religious authorities curbed (1983, 3: 134–6).

Hume also identifies a second danger from ideological parties: an intolerance traceable to the aspiration to orthodoxy that speculative reason inspires. As with religious authorities, Christianity itself is the historical agent that unleashes these aspirations. As Hume tells it, Christianity took the form of a "system of speculative opinions" to defend itself in a time when "philosophy was widely spread over the world" (1987a, 62; cf. 1988, 322; 1956, 53–4). In this way, it became an independent philosophical system and, thereby, the first intolerant religion. While earlier sects had had their differences, their "traditional tales and fictions" were easily harmonized. In contrast, Christianity was a rational system that was incompatible with other systems, and such systemic absolutism made believers unwilling to live with a diversity of human beliefs.

Last but not least, Hume blames speculative philosophy for encouraging a utopian cast of mind, whose products undermine the conditions of sound political order. A case in point for Hume is the idea that government originates in consent. He criticizes this teaching for its blindness to the necessities of human life, and his essay "Of the Original Contract" (1987a, 465–87) is designed to reveal this blindness to us while stirring us to wonder what its source is. The question for Hume is why speculative rationalism leads its proponents to political understandings that so patently ignore the limits of human experience. Hume's answer is understated but unequivocal. While the love of philosophical system is not negligible, the tendency of human beings to think that a "higher" metaphysical or religious Justice shelters them from necessity paves the way to illusion in the political realm. Hume's critique of this phenomenon is evident elsewhere in his writings, particularly in his Machiavellian critique of republican theorists who take their bearings from "imaginary republics,"[4] and his critique of the otherworldly character of religious morals (1947, 220–2; 1956, 60–72; 1988, 270, 322). Both of these critiques stress how a higher Justice tends to undermine human practices by spurring human beings to think themselves protected from the necessities that are right before their eyes. Utopian and otherworldly prescriptions are built on this foundation, Hume suggests, and here too stand the advocates of consent. Inspired by a sense of cosmic support for their principles, they seek to promote political freedom and popular enlightenment but ultimately promote intolerance, faction, and disobedience (1987a, 465–87; especially 469, 471–4, 486).

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[3] Since the revival of Hume's writings by the Austrian school of phenomenology and logical positivism (Luthe 1984, 112), Hume has been cited as an authority on various irrationalisms, such as positivism, emotivism, moral subjectivism, and cultural relativism. The most recent examples of the irrationalist interpretation are Harrison (1976) and Mackie (1980). For a complete list of the irrationalist interpreters, see Capaldi (1989, 146).

[4] Hume quotes with approval Machiavelli's views on "the philosophers [and] their imagined or dreamed of republics" in "That Politics May Be Reduced to a Science" (1987a, 25, n. 15). Hume himself chastises the republicans of his time for their model of a "fine, imaginary republic, of which a man may form a plan in his closet" (1987a, 52).

## HUME'S RATIONAL POLITICAL ORDER: REASON, WILL, FREEDOM, AND OBEDIENCE

While warning against the particular unsalutary tendencies unleashed by modern speculative reasoning, Hume argues for understanding the political order as a rational entity whose purposes can be established by reason and to which men are attached through their reason. This political rationalism has been seen to contain utilitarian (Plamenatz 1963, Sabine 1950, Wolin 1954), contractarian (Gauthier 1979, Whelan 1994), and natural law elements (Forbes 1975, chapter 2; Haakonssen 1981, chapter 2; Stewart 1992, chapter 4), but at its core is a concern for establishing political freedom through rational political opinion. This concern dictates that Hume's political rationalism is ultimately governed not by the philosophical politician's aim of doctrinal precision but by the political legislator's awareness of the fragility of all political societies. Accordingly, in shaping a rational political opinion, Hume seeks to moderate freedom's inherent threat to authority and obedience, and toward this end, he presents the political order as a product of human reason not of human choice.

While Hume's political teaching is in many respects classically liberal (Balog 1990, Danford 1990, Hirshmann 1977, Kalinowski 1993, Larkin 1969, Lerner 1987, Moore 1977, Pangle 1988, Rahe 1994, Siebert 1990), it does not appeal to the traditional liberal ground of the laws of nature or the state of nature.[5] In accordance with his strict empiricism, Hume's preferred ground is mechanical *necessity*, and this preference appears to reflect a desire to dissociate political reasoning from the authorities of a teleological Nature and a superintending, providential God. Hume is much more reticent than other classical liberals, however, about the implications of this position for human freedom.[6] He asserts the primacy of the individual for politics, but he does not ground this primacy in human freedom. Instead, his scientific empiricism alone seems to justify this primacy: It supports human freedom by negating other authorities but does not recognize freedom as a first principle. According to Hume, human beings are not free by nature, rather they are governed by their "interests."[7]

Hume's differences with other classical liberals are even more apparent in his conception of civil society and government. According to Hume, the choice to form a political community is ultimately of little importance for understanding its purposes or character, because such a choice is not really a choice at all. Individuals are compelled to form civil society and government by the inadequacy of their natural situation. "Of all the animals, with which this globe is peopled, there is none towards whom nature seems, at first sight, to have excercis'd more cruelty than towards man, in the numberless wants and necessities, with which she has loaded him, and in the slender means, which she affords to the relieving these necessities" (1987b, 484). Overwhelmed by such inadequacy, human beings follow their interests, and these impel them to form civil society. Interest can be understood in this rigidly deterministic sense because the freedom to choose in the prepolitical state is severely restricted by the pressure of human neediness and the paucity of available remedies.

In keeping with this emphasis, Hume's conception of civil society stresses duties and obligations over individual rights. Indeed, it is fair to say that Hume would have us understand ourselves not as free, sovereign individuals who have created civil society and government, but as individuals lying under the obligations of justice and obedience. Yet, contrary to this emphasis, Hume sometimes seems to make the authority of these obligations contingent upon their serving one's interest (1987b, 490–4; 1987a, 38). To resolve this apparent problem, we need only recognize how the duties of justice and obligation rest ultimately on the "real and permanent" interests of human beings, which they are not free to change or determine (1987a, 38). Strictly speaking, this standard is not above individual's interests; it is not some higher or collective good (virtue) or some aggregative good that can be maximized (wealth). Instead, the real and permanent interests of humankind refers to the condition in which human beings may best follow their interests; and thus, the authority of this standard follows from people's individual interests without being reducible to them. In short, just as the authority of interest in the prepolitical condition does not imply human freedom, so the authority of interest in civil society does not imply the freedom to disobey the duties upon which society rests. In both the prepolitical and political states, the authority of interest implies not human freedom but the subordination of human beings to the necessary means of satisfying their interests.

According to this reasoning, Hume would have us understand the political order as resting on human beings' interests and the duties of justice and obedience, whose authority derives from the permanent principles of human nature. Unfortunately, however, none of this in the end is a sufficient basis for human society. This is so because once the pressing inadequacy of the original situation is removed, interest ceases to be a reliable motive to justice and obedience (1987a, 38; 1988, 205; 1987b, 534–8). While it remains in our "real and permanent" interest to be just and obedient, we now act irrationally; we are "seduced from . . . great and important, but distant interests, by the allurement of present, though often very frivolous temptations." Such is the "Frailty or Perverseness of our nature!" Accordingly, "men must . . . palliate what they cannot cure" by instituting a government "to point out the decrees of equity, to punish transgressors, to correct fraud and violence, and to oblige men, however reluctant, to consult their own real and permanent interests" (1987a, 38). The need for government to curb our irrationality might indicate that civil society cannot ultimately be founded on our interests—that it must be imposed on us. While this may be true (cf. pp. 39–40, 468–9, 471, 474), Hume thinks it need not trouble us excessively. For it takes only a "small degree of experience and observation . . . to teach us, that society cannot possibly be maintained without the authority of magistrates" (p. 480); since government can be shown to be necessary to civil society, it too can be seen to serve our real and permanent interests.

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[5] My account of Hume's political teaching focuses on the following works: 1987b, 484–567; 1988, 183–211; and 1987a, 37–41.

[6] Cf. Whelan (1994, 220): "Except for one passing reference, Hume is silent in his philosophical works with respect to the concepts of natural liberty and natural rights."

[7] Hume insists that the conventions that found society are not based on promises, a view that would elevate the importance of human choice, but on a "general sense of common interest; which sense all the members of the society express to one another, and which *induces* them to regulate their conduct by certain rules" (1987b, 489–90) [emphasis added]. Also see Hume 1987b, 491–3.

In sum, Hume would have us understand the political order as a spontaneous creation of individuals' interests, whose foundations, character, and ends can be, and should be, grasped by reason. The authority of this order for individuals is a function of its capacity to serve their interests and its reasonableness, which soon becomes imbued with moral sanction through the sentiment of humanity (1988, 202–4; 1987b, 498–501). In so rationalizing government's authority, Hume encourages human beings to understand it in relation to their interests, and to share in the reason underlying it. In this way, he clearly discourages them from an undiscerning reverence for authority that could lead to the rule of religious authorities. But Hume also distances his conception of the political order from the human faculty of choice or will.[8] The limited character of human freedom in the natural state justifies this view, as does the practical need for obedience within civil society. In the latter case, Hume insists that obedience and a sense of obligation to the sovereign take precedence over freedom and choice, for it is necessary that human beings avoid the sense of independence that leads them to conceive their interests too narrowly or their obligations too contingently. Hume sees obedience as the pillar upon which the political order rests because it inclines people to be just: "Obedience is a new duty which must be invented to support that of Justice; . . . the types of equity must be corroborated by those of allegiance" (1987a, 38).[9] It should be noted, however, that for Hume obedience need not mean unthinking submission to every whim of government. Instead, obedience should be a rational duty, based on recognizing the necessity of justice and government to political society, and of obedience to justice and government. In this way, humans can obey unconditionally without thinking themselves slavish.

While individuals might be brought to such an understanding, Hume does not require that they be. For while resting obligations on reason helps to make individuals free, a concern for society requires giving authority and obedience precedence over rational freedom. As Hume puts it in "Of the Origin of Government," while "liberty is the perfection of civil society; . . . authority must be acknowledged essential to its very existence" (1987a, 41). Accordingly, the responsible legislator should look first to preserving obedience rather than to making us free. And because "the bulk of mankind [is] governed by authority, not reason, and never attribut[es] authority to any thing that has not the recommendation of antiquity," the wise legislator "will bear a reverence to what carries the marks of age" (p. 513). Nonetheless, for those who "have divested themselves of all superstitious reverence to names and authority" and seek to know the origin of government, as Hume thought the people of his time had and did, he offers his reasoned account of the origin of civil society and government (p. 51).

## PERFECTING SOCIETY THROUGH OPINION

The preceding section ended by indicating the role legislative prudence should play in shaping the self-understanding of political societies. Hume argues that such prudence is necessary to accommodate people's limited rationality as well as to insure obedience. All political societies stand to benefit from the wise exercise of such prudence—because all are founded on opinion (1987a, 32–6)—but none will benefit more than free societies. For these societies must succeed at the difficult task of establishing liberty without sacrificing the authority essential to the existence of all societies. The remainder of this study is devoted to setting forth Hume's conception of how this task can be accomplished through the deft formation of political opinion.

It must first be determined what the civil liberty is that Hume seeks to secure. His position is that civil liberty appears in a society characterized by such institutions as the rule of law and the division of power, as well as by the understandings that support them. Like Montesquieu, Hume does not rely on abstract theory to understand civil liberty, but sees its image in an actual political order, where it appears "as in a mirror" (Montesquieu 1989, 156; cf. Kristol 1987, 104–5; Mansfield 1989, 232–3). Most important for Hume, one sees in this mirror a *free people*—a people characterized by self-esteem, independence, and a certain defensiveness about their liberty, property, and persons. Hume consistently praises this spirit of liberty in his political writings, though without making it the subject of extended analysis. Indeed, instead of studying it, he would have his readers esteem it. They should "entertain a magnificent idea of the BRITISH spirit and love of liberty" (Hume 1987a, 49, 207, 9, 495), and admire the deeds of "all ENGLISH patriots" who vigorously defend "ENGLISH liberty" against an encroaching government (p. 374). This shining image of a free people is most useful to a people whose "indolence" and "ignorance" have led it to neglect its freedom (pp. 41, 96), but the significance of such freedom must be understood more universally. As

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[8] This argument ultimately dictates against seeing Hume's political theory as "contractarian" because it entails a great diminishment of the extent to which government is responsible to the governed. Yet, see the fine accounts of the contractarian elements in Hume's political theory by Whelan (1994) and David Gauthier (1979). Also see Whelan (1994, 216, 220–1) on the absence of will and sovereignty in Hume's political theory.

[9] See also the role of education, acquired habits, and political rhetoric in accustoming men to blame injustice (1988, 203; 1987b, 500); and see Hume's defense of deferring to the established order on the grounds that without such deference "you break all the bonds of civil society, and leave every man at liberty to consult his private interest, by those expedients, which his appetite, disguised under the appearance of reason, shall dictate to him" (1987a, 495–6).

Hume's remarks on ancient slavery and modern commerce make clear, this freedom is the condition in which human beings are most likely to flourish, and the expansion of freedom is the modern world's most powerful claim to superiority (pp. 383–4, 269–71; cf. 115–23).

Not surprisingly then, creating and maintaining a free and free-spirited people is the grandest task of Hume's political science. To perform this task, the constitutional legislator must come to understand the permanent principles of human nature and exploit them through devising political institutions that make it "the interest, even of bad men, to act for the public good" (Hume 1987a, 16).[10] At a deeper level, however, this legislator must seek to shape men's opinions about civil liberty—about its character, scope, and dependence on constitutional science.

## "OF COMMERCE": CIVIL LIBERTY AS MODERN POLICY

Hume's essay "Of Commerce" (1987a, 253–67) is addressed to legislators, but we should not conclude that they are its sole or principal audience. Rather, in this essay and its companion piece, "Of the Refinement in the Arts" (pp. 268–80), Hume recommends the legislator's perspective as the appropriate one for all who would reason about political affairs. In part, he must recommend this perspective because human beings are prone to take a "philosophical" view of political questions that is excessively moral (1987a, 280; cf. 268–9). Yet, in "Of Commerce" Hume observes not that there is too much ill-informed, moralistic opposition to commerce, but the opposite. Englishmen have been so impressed by the beneficence of commerce that "the greatness of state and the happiness of its subjects . . . are commonly allowed to be inseparable with regard to commerce" (p. 255). Hume's criticism is not that this "maxim" is false—indeed "Of Commerce" seeks to demonstrate its truth—but that it is accepted "with too little reservation and limitation" (p. 255). Because human beings are "variable," one should not suppose the maxim will always be true, for "what may be true, while [man] adheres to one way of thinking, will be found false, when he has embraced an opposite set of manners and opinions" (pp. 255–6). Reviewing these arguments helps us to see that Hume's objective in "Of Commerce" is not principally to defend commerce against moral opponents or to educate the legislator, but rather to bring his readers to appreciate the legislator's view of commerce, or, in other words, to appreciate commerce as "modern policy." In this way as well, Hume seeks to bring his readers to an understanding of the character and scope of their liberty in commercial society.

Because Hume's "Of Commerce" is presented as a teaching for legislators, we must work through this teaching to see the essay's implications for civil liberty. Hume makes two arguments for the modern policy of encouraging commerce and manufactures. First, he argues that circumstances have changed since the time of anticommercial classical republics, and that "sovereigns must take mankind as they find them"; they should not "introduce any violent change in their principles and ways of thinking." Second, he argues that the legislator should follow human nature, for "the less natural any set of principles are, which support a particular society, the more difficulty will a legislator meet with in raising and cultivating them" (1987a, 260). In the remainder of the essay, Hume moves between these two arguments instead of arguing solely from human nature. In using the argument from historical circumstances, he further indicates his unwillingness to abandon the legislator's perspective. We will focus, however, on the implications for civil liberty of Hume's argument from human nature.

This argument strongly encourages the legislator to make wise use of human passions. The bedrock of this counsel is that "every thing in the world is purchased by labour; and our passions are the only causes of labour" (Hume 1987a, 261). A wise legislator will study the passions and learn how best to exploit them. This counsel may not advocate a policy of encouraging commerce under all circumstances. Indeed, Hume thinks the ancient legislators responded correctly to the circumstances of small, mutually hostile, neighboring states when they opposed the private, selfish passions that underlie commerce. Yet, the very peculiarity of ancient circumstances and ancient policy's dependence on them points to the superiority of a policy founded on the more reliable basis of self-interested passions (p. 261).[11] A passion for public good is just "too disinterested and too difficult to support" (p. 263). Accordingly, it is the "best policy to comply with the common bent of mankind, and give it all the improvements of which it is susceptible" (p. 260).

Our central concern, however, is with Hume's argument that the modern policy of complying with the common bent is superior to the ancient policy of opposing it because the modern approach is *nonviolent*. This argument runs through the entire essay and is absolutely critical to Hume's overall comparison of ancient and modern policy and his recommendation of the latter (1987a, 259, 260, 262–3). In "Of Commerce," *violent* signifies three separate but interrelated features of ancient policy: (1) its opposition to men's passions, (2) its universalism, and (3) its willingness to "aggrandize the public by the poverty of individuals" (p. 260). In contrast, nonviolent modern policy follows the bent of human passions ("Furnish him with manufactures and commodities and he will do it of himself" [p. 262]), encourages flexible responses to particular circumstances, and balances more equitably the ends of greatness of state and private happiness. Of particular importance to this comparison is the status of moral perfection within the respective policies. Hume attributes many of the pernicious features of ancient policy to the ancient legislator's predominant concern with moral perfection, including this policy's preference for coercive force over using the passions, its rigid universalism, and its vast downgrading of individual freedom in favor of public splendor (pp. 269, 279). In contrast, he recommends a nonviolent modern policy that elevates political utility and individual freedom at the expense of moral perfection.

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[10] This ambition to understand and exploit the permanent principles of human nature accounts in large part for the eighteenth-century development of the historical sciences. See Becker 1951; Davis 1962, 213–29; Nadel 1964, 445–70; Stomberg 1951, 295–304.

[11] See Hume's argument that "the less natural any set of principles are, which support a particular society, the more difficulty will a legislator meet with in raising and cultivating them" (1987a, 260).

This point brings us to the theme of civil liberty, for it is now apparent that an enhanced respect for individual freedom is an important aspect of modern policy. What form does this respect take? Modern policy does not respect the desire to be governed only by governments to which one gives one's consent. Instead, it respects individual freedom by allowing human beings as great a sense of independence and self-rule as is consistent with the need for government. Modern policy achieves this by allowing human beings to follow their passions, and just as importantly, by interfering as little as possible with the sense of freedom that comes with following their passions, for this sense underlies the energy and confidence that makes them so productive. Stated more positively, modern policy seeks to encourage a sense of independence, self-esteem, and security, and it charges the legislator with studying human passions to find the best ways of governing indirectly, that is, invisibly. One example Hume gives of a good modern policy is a tax levied on consumption, which is both "insensible" and in some sense "voluntary." Because the tax is "confounded with the natural price of the commodity," it will be scarcely perceived; because a "man may chuse how far he will use the commodity which is taxed," he will view the tax as something linked to his choice and not as something imposed on him (Hume 1987a, 345, 265).

In "Of Commerce" Hume thus encourages his readers to understand their liberty as a feature of commercial society, whose logic is rationalized and presented as modern policy. In this way, Hume encourages them to see their society as worthy of their choice, while carefully shaping their understanding of this choice. The proper mode of choosing is not that of the consenter but that of the constitutional legislator. The latter differs from the former because it diminishes the willful element of choice by limiting and directing it. Choice is not about whether the individual consents but whether modern policy makes sense. Indeed, "Of Commerce" does not so much provoke its readers to reject or affirm commercial society as it summons them to share in its reason.

Similarly, by divorcing civil liberty from willful choice or consent, Hume divorces it from a demand to rule. Hume's civil liberty encourages people to see their liberty in their independence and security, rather than in ruling. Modern policy encourages this view by governing invisibly, and by elevating the "authority and consideration [of] that middling rank of men, who are the best and firmest basis of public liberty." Indeed, this "rank of men" comes to personify Hume's civil liberty because it does not seek to govern but to secure its property against monarchical and aristocratic tyranny by supporting "equal laws." This rank is the best and firmest basis of public liberty because it seeks only the independence and security demanded by its self-respect and has no "hopes of tyrannizing over others" (Hume 1987a, 277–8).

## CIVIL LIBERTY AND FREE GOVERNMENT

As the preceding discussion indicates, for Hume, free government consists in the rule of law, which demands some popular involvement in government.[12] In turn, free government supports a free people by encouraging qualities (a sense of independence, self-esteem, and security) that also support the rule of law. This is the condition of civil liberty, which Hume's constitutionalism achieves through modeling institutions and establishing its peculiar "civil liberty" in opinion. Yet, the need for free government to be in some measure popular complicates matters, because after opening the door to the popular principle, constitutional science may be overwhelmed by it. Hume's account of the behavior of the ancient democracies makes it clear that he was not unaware of this risk, for these democracies give ample testimony to the conflict between the will that demands civil liberty and the reason charged with securing it. As he puts it, "These people were extremely fond of liberty; but [they] seem not to have understood it very well," (1987a, 369).[13]

In his essays, Hume presents two free governments: the mixed government with republican and monarchical "parts," and the new large constitutional republic. He presents both with an eye to shaping opinion about the British Constitution, and he frequently responds to particular developments or changes in its framework. Hume clearly thinks the constitutional arrangements of 1689 and 1714 no longer enjoy a secure foundation in opinion, and in many of his essays he seeks to secure this foundation by transforming it.[14] Because the monarchy is more endangered than the republic, Hume tends to defend it, but not uncritically (1987a, 42–6, 47–53, 493–501, 502–11). Indeed, while he clearly fears the arrival of a "pure" republic (pp. 52–3), he credits republics for most of the advances in modern government (p. 125). Accordingly, the people play important roles in both of his free governments. Nonetheless, he keeps a close rein on the popular principle by stressing that civil liberty depends on recognizing constitutional science as the sole and supreme authority.

Hume writes in "Of the Liberty of the Press": "Nothing is more apt to surprize a foreigner than *the extreme liberty*, which we enjoy in this country, of communicating whatever we please to the public, and of openly censuring every measure, entered into by the king or his ministers" (1987a, 9). With these words, Hume begins his account of English liberty and then explains the defining principle of its mixed government: *jealousy*. First and foremost, jealousy demands a government of two independent "parts." In "pure" republics, the people cannot be jealous of the government because it is not separate from them; in pure republics the government is *of* the people. A similar point can be made about pure monarchy. Jealousy is not possible under pure monarchy because the monarch holds absolute title to rule and the people are understood solely in terms of their subjecthood. Under pure monarchy, the people lack the standing to be jealous; at best they can experience a "species of liberty," one granted by the monarch as parents grant liberty to children (pp. 10–1).

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[12] On the connection between the rule of law and republican government, see Hume 1987a, 115–8.

[13] See Hume's description of how the popular will of Athens controlled, and rendered ineffective, efforts to improve the government. To their credit, the Athenians recognized the flaws in their democratic constitution, but they refused to change it because they were "averse to checking themselves by any rule or restriction." They thus began to prosecute leaders of the assembly for bad policies, a measure aimed at keeping control and correcting their own folly and injustice (1987a, 408; 367–70).

[14] Cf. Forbes: Hume's philosophical politics should be "viewed as an attempt to give the established Hanoverian regime a proper intellectual foundation" (1975, x). While Hume's intent was conservative, he clearly thought conservation required innovation. This dilemma partially accounts for Hume's reputation as both a conservative and a progressive. See, as Danford (1990) points out, his inclusion in the catalog of conservatives in Anthony Quinton 1978, and among the progressives in Caton 1988.

Jealousy thus demands that the people be recognized as a "part" of the governing authority within the society. At the same time, it does not require that they govern; indeed under mixed government, they are governed by the other part of the governing authority. For Hume, under mixed government, the "monarchical part" rules, and the "republican part" jealously demands rule according to "general and inflexible laws" (1987a, 12).[15] Through this arrangement, the people actually enjoy greater liberty than they do when they govern themselves, for where government is truly of the people, it tends to be trusted with "large discretionary powers." Similarly, pure popular government tends to infringe upon liberty because, in being of the people, its authority over them is shaky, and it must thus demand that "every private citizen pay a great respect to the government" (p. 12). In short, for Hume, England's mixed government is the freest existing government because its republican element secures the rule of law while remaining subordinate to the constitution as a whole. In the latter respect, England's remarkable political freedom stems from the supremacy of the constitution's authority, which keeps government limited by keeping the people from seeing it as an extension of themselves.

From the point of view of constitutional science, however, mixed government suffers from a potentially debilitating malady. It works only when the monarchical and republican parts are strong enough to play their respective roles. The immediate historical problem for Hume was that the monarchical part was no longer up to playing its customary role. In his essays, Hume attributes this failure to three principal sources: (1) the relative weakness of the monarch's formal powers in comparison to those of the newly resurgent House of Commons (1987a, 44), (2) a change in opinion that divests the people of "all superstitious reverence to names and authority" (p. 51), and (3) the monarchy's unfortunate reliance on birth to produce "the capacity and virtue" necessary to check the republican part (pp. 46, 572). To uphold the monarchy, Hume seeks to formalize the extraformal mechanisms of patronage and influence (pp. 42–6), and to buttress the monarchy by putting the whole constitution on the new footing of constitutional science (pp. 14–31). He also calls for a "coalition" government in which the monarchist party and the republican party would moderate their opposition to one another and reestablish the existing order on the ground of a shared understanding (pp. 493–501).

Nonetheless, Hume is not certain that the malady affecting mixed government can be cured, and this uncertainty appears to be what leads him to present cautiously his "Perfect Commonwealth."[16] This form of government obviates the constitutional problems created by a weak monarchy, but more impressively, it obviates the main problem of republics: popular willfulness. In the past, republics had grown up in cities whose very smallness helped to foster common opinions, economic equality, and a nearness of habitation that bred mutual devotion. But because they were small, these republics invited the popular will to dominate: The nearness of habitation encouraged the people to feel their strength, and the force of the popular claim was "very sensible" (Hume 1987a, 528). Unfortunately, this state of affairs spelled disaster for republics; popular willfulness made them vulnerable to demagogues, who preyed on the people's envy of the elites to sow faction and violence, undermine law, and dash stability (pp. 16, 19–20, 366–70, 406–16, 472).

To oppose republican willfulness, however, is to oppose republicanism, but this is precisely what Hume does in formulating a constitutional republic that is constitutional rather than republican. In its multiple political divisions, multiple electoral tiers, lengthy and specific voting requirements, detailed designation of powers, complex legislative process, veto procedures, removal power and court of competitors, Hume's constitutional republic stresses the sovereignty of constitutional science, not republican choice (1987a, 516–22). Similarly, within this scheme popular authority and will are carefully limited, and those who govern are insulated so that "the public interest may be pursued with some method and constancy" (p. 36). But most instructive about the character and spirit of this government is its central idea. Unlike James Madison in *The Federalist* 10 (1961, 83), Hume does not locate that idea in a tendency that is at least nominally republican. For Madison, the tendency is for the large republic to create particularly broad-minded majorities that govern with an eye to the public good, while respecting the rights of the minority. Instead the genius of Hume's new order lies in the *combination* of largeness and representation that divides the people into disparate "parts" and thereby erects powerful dikes against "the force of popular currents and tides" (1987a, 36, 528).[17]

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[15] For discussions of this view of the relation between the monarchical and the republican part, see Gwyn 1965, 10, 29–30, 32, 42, 58, 75, 78, 113–4; Mansfield 1989, 158–64; and Wood 1969, 26.

[16] Hume's concern for the established form of government dictates that he advance this commonwealth solely as an "Idea of a Perfect Commonwealth" ([1752] 1987a, 512–29). But a brief reference to it in an earlier essay, "Of the First Principles of Government" ([1741], 1987a, 32–6) indicates its long-standing appeal for Hume.

[17] That Hume sees the genius of the large republic in the combination of largeness and representation is apparent from his attack on direct democracy in "That Politics May Be Reduced to a Science" (1987a, 16; cf. p. 36 and his scheme in "Perfect Commonwealth," pp. 512–29). On Hume's effort to divorce civil liberty from republican or popular

## CONSTITUTIONAL SCIENCE AND POPULAR SOVEREIGNTY

Hume's large constitutional republic is an important landmark on the road to modern constitutional democracy. Yet, our analysis has shown the great distance that must still be traveled to reach constitutional democracy's approach to politics and governance. For Hume's constitutionalism recognizes and seeks to establish the sovereignty of constitutional science, presenting it as both the source and vehicle of civil liberty. By contrast, it never admits the claim of popular sovereignty, which is crucial because it prevents Hume's call for a popular role in government from being understood as a call for popular rule. Thus, while his constitutional approach aims at establishing an order of freedom that benefits all, the people's role within it is strictly limited. Hume's constitutionalism invites the people to share in the reason of the constitutional legislator; it encourages their independence, self-esteem, and security; it even calls upon them to make their order work; but it does not let them rule.

Because Hume's constitutionalism is not guided by popular sovereignty, it helps us to see more clearly the nature of the marriage of constitutional science and democracy in constitutional democracy. The success of this marriage depends largely upon keeping the people's sense of self-rule from becoming an obstacle to their respecting a constitutional wisdom that they did not originate but that defines their constitutional arrangement. We may now observe how the marriage is sustained by presenting this wisdom in a way that least offends the people's sense of self-rule. Under constitutional democracy, this is accomplished by presenting constitutional wisdom as the product of a disembodied science, reason, or prudence, whose dictates are available to all at little cost to their sense of self-rule. In this way, modern constitutionalism can become a tool whose maker is easily forgotten.[18] Compare this conception with Hume's association of constitutional wisdom with the authority of the legislator. Through the persona of the legislator, Hume elevates constitutional wisdom above the people while inviting them to share in it. He encourages them to adopt the legislator's perspective but suggests that they must ultimately defer to a superior reason. Legislators deserve a place of honor in society: "Of all men, that distinguish themselves by memorable achievements, the first place of honor seems due to LEGISLATORS and founders of states, who transmit a system of laws and institutions to secure the peace, happiness, and liberty of future generations" (1987a, 54).[19]

While encouraging us to forget our dependence on superior reason, constitutional democracy also tends to supplant respect for constitutional wisdom with reverence for the constitution as law. To be sure, all constitutionalists—Hume included—will encourage a "suitable degree of zeal" for constitutional forms, as well as the reverence and veneration born of time and the forgetting of imperfections. The constitutionalist is always aware that all governments rest on opinion, and that the strength of any opinion depends as much on its age and number of adherents as it does on its correspondence to reason. But democracy further encourages us to revere the constitution as law because in the name of freedom it demands the rule of laws rather than of individuals. Indeed, it is a mark of the genius of constitutional democracy that it guards individual freedom and independence by hiding their reliance on appropriated constitutional wisdom. Yet, in severing freedom from such wisdom, might not constitutional democracy be depriving it of its best defense?

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government, also see Miller 1990, 101. Compare an alternative reading of Hume's political agenda as serving more republican ends, such as "responsible government" in Stewart 1992, 251–7; and an alternative account of Hume's argument for the extended republic that stresses the continuity between Hume and Madison made by Adair (1974, 93–106).

[18] See especially the references to "prudence" and "science" in *The Federalist Papers*, as listed in Engeman 1988, 440, 484. On Publius's use of the authority of Montesquieu, see Kristol 1987, 102–7.

[19] Compare Madison's treatment of the legislator in *Federalist* 38 (1961, 231–40).

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# Serving God and Mammon: The Lockean Sympathy in Early American Political Thought

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*This paper seeks to revive the old theory of a "Lockean consensus" in early American political thought against the prevailing "republican" view. The language of "virtue" and "slavery," which was pervasive at the time of the founding, and which many have been eager to take as evidence for the influence of civic humanism, in fact has a perfectly plain Lockean provenance. This is established first through a reexamination of Locke that links his account of virtue to a Christian asceticism (i.e., the Protestant Ethic) rather than republican philosophy. That the founders understood virtue in this way is then established through an exploration of Adams and Jefferson. In both cases, it was a Lockean slavery which they feared and a Lockean virtue which they sought. A Lockean sympathy did exist among the founders; in order to understand it, however, it must be distinguished from modern liberalism, with which it has only tenuous connections.*

> The task of the cultural analyst is not to discover simplicity, or even to discover unity, for simplicity and unity do not exist, but to drive a wedge of rationality through the pathetic indecisions of social thought.
>
> —Louis Hartz

The founders' political and constitutional theory has undergone a remarkable transformation in the last 25 years. Although the authors of that theory are long dead, various interpreters have posthumously rendered "republican" what was once widely assumed to be a "liberal" creed. The republican thesis, at first tentatively advanced as a corrective to an overweening historical consensus around liberalism (Bailyn 1967; Wood 1969), for some writers has recently taken on the trappings of a fundamental paradigm (Elkins and McKitrick 1993; Pocock 1975). It has proven enormously popular, both with historians and political scientists as well as with legal scholars. The implications of this historical thesis are profound for almost any study of the American past. According to proponents of republicanism, we fundamentally misunderstand the political theory that underlies the U.S. Constitution and the political system it grounds if we focus on traditional liberal concepts like individualism and property. We would do better, they argue, to look to the language of civic virtue "anchored in th[e] Aristotelian and Machiavellian tradition" that allegedly permeated the rhetoric of that generation which wrote the founding documents and first inhabited the institutions those documents created (Pocock 1975, 506). "There was," J.G.A. Pocock has gone so far as to claim, "no alternative tradition in which to be schooled" (p. 507).[1]

But what of the older theory of Lockean consensus? Is its historical usefulness at an end? This essay argues to the contrary. When properly tempered, the Lockean perspective on the founding still has much to teach us. Although the excellent work done by republican historians must surely moderate the grandiose claims once made on behalf of Lockean liberalism, it will be argued below that it is still more correct to call the political theory of the founders Lockean rather than republican as we use these terms today. I begin by arguing that the collapse of the Lockean consensus theory (so named by Louis Hartz but hardly invented by him) is due as much to the reassessment of Locke that took place in the 1950s and 1960s as to a reappraisal of the founders' political theory. It was this shift (briefly described in the second section) which increasingly laid open to doubt the understanding of American politics as Lockean.

What the bulk of the essay provides, then, is a reinterpretation of Locke which renders him a more plausible figure for an early American consensus (in the third section), and then a verification that Locke did indeed play this role through an exploration of two major revolutionary figures, Jefferson and Adams (in the fourth section). The aim is to defend a chastened version of the Lockean consensus theory, which I have labeled the "Lockean sympathy." The theory is chastened in that its breadth is meant to be restricted in two ways. First, the historical work of republican scholars (and many others) requires that we not assume the founders, or any generation of Americans, spoke with a single voice. Second, the founders' Lockeanism cannot be abruptly equated with twentieth-century liberalism, as some inheritors of Hartz have maintained.[2] Although there are certainly threads of connection between Lockean liberalism and the modern varieties, there are just as many

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[1] Pocock would probably no longer claim quite so much for his interpretation, but the quotation does give a sense of the tone with which republicanism has been and still is championed (by some, e.g., Elkins and McKitrick 1993).

[2] Hartz himself did not make this simple equation. His work, however, with its broad characterizations of American liberalism, perhaps encouraged others to do so; it is therefore necessary to counter that tendency here. Hartz's subtle analysis of the evolution of American liberalism has, unfortunately, often dropped out of subsequent uses of his terminology. I do not have space here to give Hartz's work the full appreciation it deserves. See Hartz 1955 and 1964 as well as Diggins 1988, Horowitz 1978, and Riley 1988. I thank an anonymous referee for bringing this point home to me, as well as for the Horowitz reference.

that have been cut. The most important of the latter, I argue below, is the strong Protestant element running through both Locke's philosophy and that of the founders. It is only through an appreciation of the Puritan roots of Locke's political theory that we will be able to perceive its links to that of the founders, even if the result is that we end up feeling more distant from both.

One of the major premises of this argument, which it will be the burden of the third section in particular to establish, is that the language of virtue and slavery, which some historians have been eager to attribute to the reading of the Latin classics "refracted" through such Renaissance figures as Machiavelli (Wood 1969, 53, 50) has, in fact, a perfectly plain Lockean provenance. This fact has been obscured by certain interpreters of Locke (both pro and contra) who have been eager to assimilate him to modern free-market theories, but it is clear enough upon careful examination of Locke's main texts. In arguing that the founders were Lockean, then, it is not necessary to deem them simply proto-capitalists. It is instead a matter of understanding the sources of the founders' often self-sacrificing theory in Locke's Christian liberalism. Modern historians have been too eager to find in every antipossessive individualist utterance of the founders clear evidence of republican political theory (Wood 1969, 53ff.). Indeed, they have defined republican virtue (said to be the link between the founders and classical theorists) as the "willingness of the individual to sacrifice his private interests for the good of the community" (Wood 1969, 68; see Pocock 1975, 184). But the founders' defense of self-sacrifice and unselfish patriotism has clear roots in Christian asceticism, which is at the heart of Locke's liberalism as well. As I argue below, however, self-sacrifice, in this context, does not stem from polis-centered public-mindedness (as in republican thought) but from an inward-looking ideal of self-denial. It is not necessary to trace the founders' notions of virtue and self-denial tortuously backward through several layers of English political thought to vague connections to fifteenth-century Florentine philosophy. Rather, these themes can be seen to stem clearly and directly from Locke and his severe Protestantism, which the founders often shared. By illustrating, first, then, that for Locke, individualism, virtue, and ascetic Christianity were not only compatible but also deeply connected and, second, that Jefferson and Adams argued similarly, this essay aims to show that Locke provides the best lens through which to understand the political theory of the founders.

We are long past the time, however, when we might hope to find American political thought distilled in the mind of any one thinker, even an American. The early Americans were not programmed with a unified political theory by a language paradigm or any single writer. The explosion of historical work on the Revolutionary era has, at the very least, made it impossible to characterize in overly simple terms the multifarious political culture of that time. And yet neither can we abandon our efforts to make some sense of the founding period, since our inheritance from that time includes the fundamental political institutions we still inhabit. It is the question of the character of these institutions and the political theory that undergirds them which is at issue here. If we throw up our hands in despair at the welter of perspectives apparent in that generation, we rob ourselves of a significant degree of self-understanding. It is for this reason that Hartz exhorted us to persevere through the complexities of the world in order to make some sense of it. Although I conclude that his hypothesis of a Lockean consensus was too strong, I believe that it had a germ of truth to it. I have chosen to call that germ of truth a Lockean sympathy.[3]

## THE DEMISE OF A HIDDEN CONSENSUS

When Hartz (1955) wrote that American politics operated within a "Lockian" consensus (as he spelled it), he did not break new ground as far as the founders were concerned. If anything, his work was a sort of culmination of an historiographic tradition that associated Locke with the American founding (Baldwin 1928; Barker 1948; Becker 1922; Curti 1939; Miller 1943; Parrington 1927; Rossiter 1953; and, similarly, Beard 1935). Yet, within a generation the theory of Lockean consensus had been largely replaced by the theory of republicanism. Both historians and political theorists came to believe that the central ideology of the Revolutionaries had been one of civic virtue (Bailyn 1967, chapter 2; Pocock 1975, chapter 13–5; Wood [1969] 1972, chapter 2) and that the idea of a preponderant Lockeanism was a "myth" (Pocock 1980). "It can be shown," Pocock has written, "that the American Revolution and Constitution in some sense form the last act of the civic Renaissance" (p. 462). How could the world of American letters turn so quickly from being absolutely convinced of Locke's centrality for the Revolutionaries to being certain of his irrelevance for them? The answer depends, I believe, on the existence of not one but two Lockean consensuses. Within Hartz's thesis, there was a second, inner Lockean consensus, holding up the historiographic theory. When this inner consensus collapsed, the outer one soon fell with it.

The inner consensus to which I refer is an agreement, not about American politics or within American politics, but about Locke himself. At the time Hartz (1955) wrote, there was widespread accord about the meaning of John Locke's writings and the nature of his political theory. In a sense, this is attested to by a curious gap in Hartz's book—the absence of any sustained analysis of Locke himself. It may seem odd that a book which intends to describe American political culture through the icon of a single political theorist would not pause to consider the writer's work in some detail. But in fact, Hartz never bothers with a full account of Locke's philosophy; he only stops to remind us briefly, from time to time, of some element, as if he merely needs to jog our memory on the subject. The reason for this silence is contained in the book's thesis itself: "Locke dominates American political thought, as no thinker anywhere dominates the political thought of a nation. He is a massive national cliché" (p. 140). Hartz felt so comfortable that he already shared an account of Locke with his readers that he saw no need to repeat it; if Locke required explanation, Hartz's case would thereby be weakened. Shortly after Hartz wrote, however, this hidden Lockean consensus on which he relied came under attack.

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[3] *Ethos* or *consensus* each imply a strong or enduring commonality which I do not believe the evidence warrants. I therefore choose to characterize the Lockean tendency as a common pathos among some of the most important early American political thinkers. A *pathos* is something less all-consuming than an ethos or a consensus; it does not exclude the possibility of other *pathē*, even though it may be temporarily more intense. *Sympathy* thus denotes the common pathos that I find among the Americans, something weaker than Hartz postulated, but still there.

Already in the early 1950s, two caustic studies of Locke had appeared (Macpherson 1951, 1954; Strauss 1953) that were deeply critical of the consensus image of Locke. The strategy of both Leo Strauss and C. B. Macpherson was to assimilate the political theory of Locke to that of Hobbes. For Strauss, the connection was to be made through the concepts of atheism (p. 220) and hedonism (p. 249), both of which, he contended, Locke secretly advocated beneath a veneer of natural law (p. 206ff.). For Macpherson, the common denominator was a rapacious capitalism (1962, 269ff.) which led to rank inequalities of wealth and power (p. 221). In both cases, the idea was to discredit Locke by an association with Hobbes's authoritarianism (Macpherson 1962, 270; Strauss 1953, 232). Although these attacks were not universally accepted, they marked the beginning of the end of the inner Lockean consensus.[4] This left theorists of American political culture without a fixed icon around which to paint their picture. When the core image of Locke began to seem hollow, it became apparent that Hartz's account of American politics lacked a clear reference point from which to start. The storybook Locke disappeared in a cloud of controversy, dragging Hartz's "storybook history" along with it (Hartz 1955, 3).

These new interpretations of Locke were seen as extreme and provocative by many. They were met with strong counterarguments (Berlin 1964; Dunn 1968; Ryan 1965; Seliger 1963). But these replies did little to restore the image of Locke as a sound basis for an American political consensus. Instead, while rescuing Locke from Strauss's claims of atheism and Macpherson's of capitalism, they made him into a figure too remote to be of much consequence to most Americans, colonial or contemporary. The most important of these responses was that contained in the work of John Dunn (1969a), which relied on extensive research with previously understudied Lockean manuscripts in Oxford. To the hedonistic Locke of Strauss and the capitalistic Locke of Macpherson, Dunn counterpoised a religious, almost monkish, Locke whose arguments rested "for their very intelligibility, let alone plausibility, on a series of theological commitments" (Dunn 1969a, xi). But the religious roots of Locke's thought, while countering the claims of Strauss and Macpherson (or so Dunn thought), lent little comfort to a Hartzian perspective. In Dunn's view, "the only sort of social structure subject to rational resistance as a whole [for Locke] is one which claims rights over individuals which are formally incompatible with their discharge of Christian duties or which explicitly denies the religious equality of all men" (p. 265). Although the motives of the American revolutionaries have always been disputed and were no doubt various, rarely is religion ranked paramount among them. So Dunn's refutation of Strauss and Macpherson, even among those who accepted it, did little to revive the notion of a Lockean consensus in American politics (at least initially). Indeed, by rendering Locke an almost medieval figure, Dunn, if anything, contributed to the decline of Lockean consensus as an historiographic theory.

What had been a quiet, unremarkable consensus of political theorists about Locke prior to Hartz's writing degenerated, with remarkable speed, into a variety of bitterly divided camps, a situation which still obtains today. In the wake of the collapse of the inner Lockean consensus, the outer, historiographic consensus, which had been built on top of it, soon gave ground. Without Locke as an agreed-upon placeholder, the interpretation of the American Revolution as a "liberal" event made less sense. The core of this liberalism could not be readily identified so long as Locke's meaning remained contentious. While the demise of the inner Lockean consensus doubtless did not force a revision of American historiography, the confusion about Locke certainly lowered the barriers to such a revision.

The republican thesis put forward by a series of historians starting in the 1960s, then, did not so much have to defeat an opposing view as step into the vacuum rapidly developing where Lockean consensus had once stood.[5] To this was added a wealth of detailed historical analysis. In particular, the careful reading of previously obscure colonial pamphlets and newspapers in Wood ([1969] 1972) demonstrated that the common political vocabulary of that era was very different from our own. Although concerned about property and taxes, the early Americans spoke of "virtue," "corruption," and "slavery" to a degree and in a manner that had gone unrecognized in the work of earlier historians. More specifically, they spoke of virtue as the quality possessed by both the good polity and the good citizen and of corruption and slavery as forces which could lead to the degeneration of either. From a Hartzian perspective, this language seemed distinctly unLockean. To many, this evidence was thus the final blow to an historiographic tradition already severely weakened by the theorists' squabble over Locke.

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[4] To be sure, Strauss and Macpherson did not themselves contest the Lockean consensus theory; indeed, they sought to *exploit* it, that is, to indict American culture (or Anglo-American culture) via an indictment of Locke. It was perhaps an unintended result of this critique that, in order to preserve a positive image of their political culture, some analysts turned to the republican tradition in an attempt to put distance between Anglo-American politics and Locke.

[5] This, too, is attested to by a curious absence, namely, the absence in Bailyn and Wood of any explicit discussion of Hartz, despite the fact that it is Hartz's thesis against which, they signal by many means, they are arguing. Hartz appears in neither Bailyn's nor Wood's index. Pocock, of course, does mention the Lockean consensus theory, but only to ridicule it. He also specifically cites the revisionist interpretations of Locke as a reason for ignoring him (e.g., Pocock 1975, 424, 509; Pocock 1980). More recent historical writing, like that of Elkins and McKitrick (1993), then takes its bearings entirely from Bailyn, Wood, and Pocock and does not deem it necessary to mention other historiographic traditions (chapter 1, p. 5ff).

Recently, however, the republican view has begun to elicit criticism. A number of scholars have begun to argue that the republican historians have, at the very least, overstated their case and, at worst, distorted it (Appleby 1978, 1986, 1992; Diggins 1984; Dworetz 1990; Huyler 1995; Kramnick 1990; Shain 1994). Not coincidently, many (although not all) of these same rebutters have also insisted on the primacy of Locke in the political theory of the founders. But this most recent generation of scholars has, it seems to me, still failed to overcome the problem created by the demise of the inner Lockean consensus. While they point to the founders' constant references to "the incomparable Locke" or note the frequency with which Locke was cited, they have as yet been unable to reconstruct a Locke who is a plausible candidate for an American political consensus. They have been unable to shake the grip of Macpherson, Strauss, and Dunn and arrive at a Locke who would have been recognizable to the founders.[6]

Macpherson and Strauss, although they agreed on little, agreed on one thing: Locke's defense of property could not be reconciled with his apparent Christianity. Both concluded that the Christianity was, in Macpherson's term, a "facade" (Macpherson 1962, 270). For Strauss, this was a purposeful deception on Locke's part, meant to mislead all but a select few readers (Strauss 1953, 206ff). Macpherson merely felt that Locke was trying to soften his words for all his readers (Macpherson 1962, 270). Neither took seriously the idea that Locke might have been in earnest as much about his God as about his Mammon. Dunn's view was much the same, only the poles were reversed: Locke was serious about his Christianity; the charge that he endorsed a rapacious capitalism was thereby refuted. But this supposed incompatibility of the two creeds on which all three interpretations are premised is not found anywhere in Locke's thought. It seems merely that none of these interpreters could quite bring himself to believe (as I argue below) that a devout Puritan could, at the same time as he defended a radical spiritual egalitarianism, defend also an unfettered property right and the rank inequalities of wealth that would result from such a right. Yet, such a combination is not really rare at all. Max Weber (1920) described it nearly seventy years ago as a frame of mind that generations of Europeans possessed: the Protestant Ethic.

The false dichotomy of God and Mammon has had a considerable effect on the historical literature in the sense that, whenever historians identify Puritan sources of revolutionary theory, they offer them as something that cuts against (or offers no support for) a Lockean interpretation of that theory (Diggins 1984, 7–9; Shain 1994; Sheldon 1991, 161; Wood 1969, 114–8). Even among those who contest the republican approach, the presumed division between religion and commerce remains paramount. Thus, while Dworetz (1990) represents one of the most consistent efforts to revive the notion of a Lockean consensus, he does so from entirely within the context of Dunn's interpretation. As a consequence, Dworetz has accepted severe restrictions to what can be claimed on behalf of Locke: He limits his substantive inquiry to the New England clergy and does not discuss in detail any of the major figures of the Revolution. Similarly, Pangle (1988) (and to a lesser extent Zuckert 1994) attempts to make the case for the influence of an atheistic Straussian Locke with results that, necessarily, appear almost as an inverted mirror-image of those of Dworetz.[7] Although Shain (1994) makes a convincing case for the role of Protestantism in colonial American beliefs, Locke's relation to this belief system is distant at best (in Shain's telling), largely because Locke is presumed to be a liberal individualist, while the founders' Protestantism is identified as wholly communal in spirit.

Of course, a large contingent of scholars, particularly of Jefferson, have never ceased to doubt the connection between the founders and Locke (Hellenbrand 1990; Howe 1966, chapter 1; Malone 1948, passim; Miller 1988, chapter 5; Morgan 1988; Peterson 1970, chapters 2, 3; Sheldon 1991, chapter 3; but see Matthews 1984, 1995). This position is often maintained, however (even in the most sophisticated accounts, such as Morgan's), through insufficient attention to Locke himself. The revisionist accounts of Locke are not given much weight, and the Locke who remains is the simple Hartzian theorist of social contract and rebellion. While there is much to recommend this view, it is no answer to the republican historians, who point to the ubiquitous language of virtue as evidence for an unLockean outlook among the founders. What I would like to argue is that Locke *links* his social contract model to an ascetic Christian (rather than civic humanistic) account of virtue, and it is this link that attracts the founders. Here I build on the work of Kramnick, who has probably traveled the farthest in this direction in maintaining that the two strands of thought are "compatible" (1990, 275), but he continues to identify Locke largely with the former strand (see also Kloppenberg 1987).

A recent strategy has been to declare the debate moot by maintaining that there is no firm distinction between liberalism and republicanism (Banning 1992; Kloppenberg 1987; Wood 1987). Huyler's claim (1995, 147) that Locke himself was "among the seventeenth century's most devoted civic humanists, or classical republicans" perhaps represents the apotheosis of this position, although he, too, testifies usefully to the compatibility of Christianity and Locke's theory (chapter 4). But, as I hope to show below, the differences between Locke and classical republicanism are real and significant; their accounts of virtue are not easily conjoined. To paper over the contrast only returns us to a pre-Hartzian night (albeit at a higher level of complexity) in which all cows are black, that is, to a point at which it is difficult to offer any firm characterizations of American political thought.

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[6] It is important that I note here a significant exception, namely, the work of Richard Ashcraft. Although he has not attempted to link Locke to the founders, Ashcraft (1968, 1986, 1987) has gone a long way toward rebutting the interpretations of Locke that I described above. While I disagree with Ashcraft in many particulars, as will become clearer below, it should be clear that a deep debt is owed to him in righting the direction of the debate about Locke and for originating a detailed, sensible, powerful interpretation of Locke's texts which would, indeed, have been recognizable to the founders.

[7] Grant (1987), while not addressing the question of the founders' Lockeanism, does a much better job, to my mind, in rescuing the strongest parts of Strauss's view without relying on hermeneutic devices which allow one to dispose of parts of texts that do not support one's interpretation by claiming they are exoteric deceptions. As I outline below, however, my interpretation differs considerably from the Straussian in its emphasis on the religious elements in Locke's thought.

While the work of all these scholars represents a significant challenge to "storybook republicanism,"[8] it cannot really fulfill its promise until we are better able to come to grips with Locke himself. Specifically, we need to understand the special position of Locke's political theory between God and Mammon *and* the appeal of this position to the colonial Americans. Only then, I believe, will we be able fully to appreciate the founders' Lockean sympathy.

## JOHN LOCKE'S WORLDLY ASCETICISM

The first step in reinvigorating a theory of Lockean sympathy that can illuminate American political thought and the founding documents is, therefore, to locate a Locke who is at least a plausible candidate for such a consensus. In brief, I contend that many of the qualities and creeds that Locke interpreters have held to be incompatible and therefore have decided Locke could not have simultaneously held (e.g., asceticism and utilitarianism; Christianity and capitalism) were in fact combined by Locke into a single world-view. This view may not have been airtight; it relied upon certain metaphoric associations (around labor) as well as assumptions (about the nature of God) to which we no longer generally subscribe. But it was reasonably coherent. And it was (I maintain in the fourth section) not only Locke's view but also a predominant view among the founders as well.

### Locke's Protestant Ethic

***Labor.*** Part of the problem is the confusion that surrounds Locke's use of the term *labor.* We commonly use it in contradistinction to, if not simply as an antonym of, *thought.* If we use this understanding in an analysis of Locke, many "contradictions" in his philosophy immediately present themselves. This is Macpherson's strategy (1962, 200ff.). But Locke's idea of labor encompasses the mind as well as the body.[9] Thinking is a labor as surely as is physical exertion: "Man's power and its way of Operation [are] muchwhat [sic] the same in the Material and Intellectual World" (*Essay* 1975, II, 12, 1).[10] It is only by "Industry and Labor of Thought" that the frontiers of knowledge may be expanded (*Essay* IV, 3, 6). We might say that Locke's use of a vocabulary of labor to describe mental activity is "metaphorical." But this would be misleading if we were to distinguish the metaphorical use of *labor* from the "real" labor of physical activity. For it is precisely such a distinction that Locke does not make. "General and abstract ideas . . . are the Workmanship of the Mind," he writes (*Essay* IV, 12, 3). Our modern understanding of labor misleads us when it requires that labor be the opposite of thought. For Locke, labor is the very essence of thought.

Nor does the notion of a mind that labors mean that Locke's philosophy must be mechanical or antireligious as Strauss believed (1953, 249). Locke's use of *workmanship* is interesting in this regard: it is well known that Locke used this term to describe human beings, as *God's* "workmanship" (*Two Treatises* 1960, II, 6). For Locke, God himself is not above or beyond labor. God works. An understanding of man as a laboring being both in thought and action does not separate man from God but, rather, makes them more similar (see Seliger 1968, chapter 2). Our thoughts may not be God's thoughts nor our work God's work, but all these things have a similar common form in the concept of labor itself. Locke's usage demonstrates that, of itself, having labor at the center of one's political theory does not make it, thereby, simply capitalist or antireligious (although that is certainly a possibility). In fact, Locke uses labor to bind together those creeds that we often suppose to be naturally at odds. Labor, for him, is both an economic *and* a moral, even Christian, concept.

***Worldly Asceticism.*** This commonality ought to alert us that the connection between thought and labor is not simply one-way. That is, it is not simply the case that Locke has "reduced" thought to a kind of labor. He means just as much to lift the concept of labor up, as it were, and endow it with the moral status accorded to thought. Even the most severe physical labor bears a certain resemblance, in Locke's view, to God's creation of the world and of man. This is Locke's understanding of the idea that human beings are created in God's image. Both man and God are, ultimately, laborers. And it is this exaltation of labor (as well as the understanding of thought as labor) that forms the moral core of the Protestant Ethic. Work acquires a certain holiness, and a willingness to work is seen as a virtue. It is this broad conception of labor that provides the link between property and virtue; and this link is crucial to an understanding of the political theory of the founding generation.

The set of religious beliefs and practices, described by Weber (1920, 48ff.), that prescribes self-denial and hard work for otherworldly rewards is readily discernible in Locke. It appears most unabashedly in his book on education (see Kramnick 1990, 190ff.). Only a third of this book is concerned with what we would now call formal education. The first two-thirds are concerned with a kind of physical, social, and moral education that prepares one for a life filled with labor and self-sacrifice. Rather than book learning, it is the conditioning of the body and the spirit that most concern Locke. His concern is manifested in a chastening education meant to impose a severe discipline upon children from the earliest possible age.

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[8] For a more extensive survey of this literature, see Matthews (1987, 1995).

[9] What *is* present in Locke is a social distinction between those who spend their lives principally in physical labor and those who have time to reflect (mental labor). But this reflects only on Locke's "sociology," so to speak; it says little about his generic understanding of labor. See Locke (1823 "Reasonableness," 135, 146; "Money," 71; "Education," 6, 117; *Essay*, IV, 20, 2).

[10] Locke's *An Essay Concerning Human Understanding* is cited by book, chapter, and paragraph number; *Two Treatises* is cited by book and paragraph number; and "Some Thoughts Concerning Education" is cited by paragraph number. All other works by Locke are cited by page number of the referenced work.

Often the only thing remembered about Locke's writings on education is that they oppose corporal punishment. While this is true, his educational regimen is anything but sentimental; indeed, it is, by design, quite harsh. "The body," Locke notes with a dry satisfaction, "may be brought to bear almost anything" ("Education," 9). Children are not to be fed tasty foods, and their beds should be hard rather than comfortable ("Education," 13, 22). Indeed, for Locke, education seems to consist mostly of endurance tests. The ancients taught bravery through imitation (Aristotle 1941, 1103b), but when Locke repeats this advice, he gives it an ascetic gloss: "The way to harden and fortify children against fear and danger, is to accustom them to suffer pain" ("Education," 115). Locke's education tempers the body by exposing it to suffering. While he counsels against beating children, he nonetheless favors exposing them to as much discomfort as nature can provide. The target of this training is not just the body but the mind as well. Through severity Locke aims to foster good morals.

The virtue of self-denial is the principal virtue in the ascetic morality that Locke hoped to develop through education. Among his few articles of faith, a traditional asceticism is prominent: "Whoever will list himself under the banner of Christ, must, in the first place, and above all things, make war upon his own lusts and vices" ("Letters," 6). Locke's education is thus a training for war, a war upon one's own "lusts and vices." The virtue of self-denial is something only a Christ-like few possess by nature; for most, it must be bred into them through a physical and mental process of toughening. When the body has been trained to endure suffering and the mind accustomed to experiencing it, then and only then will it be possible to lead a virtuous life:

> As the strength of the body lies chiefly in being able to endure hardships, so also does that of the mind. And the great principle and foundation of all virtue and worth is placed in this, that a man is able to deny himself his own desires, cross his own inclinations, and purely follow what reason directs as best, though the appetite lean the other way ("Education," 33).

Locke does not separate mind and body when he considers where the virtues lie. What matters most is the suppression of vice. What the body does at the direction of another when young, the adult mind will practice on itself when older ("Education," 38). Virtue is neither glorious nor fun. It is a labor to suppress one's appetites; at first we are harnessed to this labor, later we harness ourselves to it.

That this ascetic ethic is connected to labor in an economic sense is clear from the account of virtue in labor that appears in Locke's economic writings.[11] Again and again, he writes of the financial rewards that result from industriousness and frugality (and not, say, genius or entrepreneurial daring). The opposite of these virtues is taken to be not merely laziness but a self-indulgence that is morally censurable. In the first of his three tracts on money, Locke tells the tale of an upright and ascetic father who builds up a vast estate only to bequeath it to a wrongly spoiled (i.e., poorly educated) son. The young man's taste for fine food, wine, and entertainment doom him to poverty and sin: "This will tumble him down the hill the faster, and the stock, which the industry, frugality and good order of his father had laid up, will be quickly brought to an end, and he fast in prison" ("Money," 20). Material rewards accrue to the laboring, worldly ascetic. And they cannot even be maintained, much less increased, by the self-indulgent, no matter how bright or bold. As Hundert puts it (1977, 40), "Locke seems to approve more of a certain personality type and a social ethic" rather than attempt a detailed defense of capitalism; but rather than representing a "secularization" of Puritan principles, as Hundert maintains (1972, 9), it is a conscious application of those principles.

Still, this distribution of riches according to moral worth is not merely meant to be of interest to the individual. Locke actually builds his whole economic theory around this idea. Although it is easy enough to label Locke's support of property and free labor "capitalist," it should also be noted that he thinks the competitive contest, even at the international level, will be won not by the strongest or the cleverest but simply the most virtuous, where virtue is strictly equated with self-denial:

> Nature has bestowed mines on several parts of the world: but their riches are only for the industrious and frugal. Whomsoever else they visit, it is with the diligent and sober only they stay; and if the virtue and provident way of living of our ancestors (content with our native conveniences of life, without the costly itch after the materials of pride and luxury from abroad) were brought into fashion and countenance again amongst us; this alone would do more to keep and increase our wealth and enrich our land, than all our paper [money] . . . ("Money," 72, cf. 53–4).

So, if Locke can be said to turn thought into labor, it can equally be said that he intellectualizes, or moralizes, labor itself. Labor has a moral aspect, and commerce, as the social result of labor, takes on that aspect as well. Thus far, then, we have a fairly good idea of what Locke favors in individual and social morality. His is a theory of virtue, but a virtue predicated on the Protestant Ethic. Now we must consider what sort of politics such a Lockean morality would favor, and what politics it would oppose.

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[11] Vaughn (1992, 125) contends that "one gets no sense of asceticism from the pages of Locke's *Considerations on Money*," but she offers no detailed reading to back up this startling declaration. In the same volume, Ashcraft (1992, 23–6) does a much better job of demonstrating how, for Locke, self-denial is one of the main attributes of reason.

## Political Freedom and Political Slavery

The concepts of freedom and slavery are closely connected, in Locke's philosophy, to his expansive notion of labor. Slavery, for him, is something that can appear in thought as well as in action. It is this account of a kind of political slavery (as opposed to chattel slavery) which, I shall argue, forms the basis of his political theory. It is also, I maintain later, a key Lockean element in the political theory of the founders.

Slavery is the political evil which occupies Locke most. It is the first word of the *First Treatise* and is there described as "so vile and miserable an Estate" as to be comparable to death (*Two Treatises* I, 1). Although Locke is capable of imagining a justified slavery, it can only be a kind of delayed death, the interval between the pronouncement of a just death sentence and its execution (*Two Treatises* II, 22–4). Locke always characterizes political oppression as a form of slavery. Although he attacks Filmer for identifying slavery with just political obligation, he is eager to consider *unjust* authority as enslavement.[12] This is something, as we shall see below, that he shares with the founding generation. Although it is often asserted that the founders derived their rhetoric of political slavery from the republican tradition, there are perfectly adequate resources within Locke's thought from which to work. While one must be careful about declaring the existence of a Lockean influence (without peering inside long-dead minds), it is certainly *not* the case that widespread rhetoric of "virtue" and "slavery" is, of itself, prima facie evidence of republican philosophy. Locke used this terminology extensively while remaining, in Pocock's words, "no kind of classical or Machiavellian republican" (Pocock 1975, 424).

But what use did Locke make of these words? What did *slavery* mean to him? It may help us to start at the other end, as it were, with his definition of *freedom* (see Schouls 1992). Since he describes, as we saw above, the moral necessity of "crossing one's desires," Locke will not describe liberty as the capacity to do whatever one likes (or even to do whatever one likes without harming others). The more fully developed the mind, in fact, the more one is able to counter one's inclinations. And it is this capacity to suspend desire, rather than an ability to choose or practice one's desires, that Locke calls free will (*Essay* II, 21, 47). Aware that this may sound paradoxical, he nonetheless insists that self-restriction is freedom, and fulfilling one's desires is slavery:

> This is so far from being a restraint or diminution of Freedom, that it is the very improvement and benefit of it: 'tis not an Abridgement, 'tis the end and use of our Liberty; and the farther we are removed from such a determination, the nearer we are to Misery and Slavery (*Essay* II, 21, 48).

The self-denial that was enforced on the child by his teacher is, for the adult, enforced on his body by his mind. Indeed, in this odd section of the *Essay*, Locke says that the mind which can deny its body the most is, on that account, both the most adult and the most free. What is prized most is independence of mind. And this notion of liberty means that the mind should not be subject, as far as possible, to the influence of physical pleasure and pain. It is this conception of freedom which allows Locke to equate such threats to liberty as the physical temptation to pleasure and the threat of injury at the hands of an absolute government: Both threaten to make concerns of the body overwhelm the freedom of thought. Both desire and absolutism threaten the mind with "slavery"—in one sense to the body itself but in a more important sense to those who can control the mind through the body, whether purveyors of sin or absolute monarchs. The Protestant Ethic allows Locke to be equally hostile to authoritarian government and physical pleasure. One does not seek freedom from absolute government in order to pursue one's wants but to fulfill one's duties.

That freedom of thought is the primary freedom is a point Locke makes repeatedly, even when he later comes to identify what ought to be respected by the government as property rights. He speaks of those under absolute rule as "degraded from the common state of Rational Creatures" and the formation of ideas as a product of liberty (*Two Treatises* II, 91; *Essay* II, 28, 7). The worst kind of subjugation is a mental one, and "he is certainly the most subjected, the most enslaved, who is so in his Understanding" (*Essay* IV, 20, 6).[13] This position is less surprising when it is considered that one of the contemporary issues which most concerned Locke was the protection of Christian dissenters from an enforced conformity to Anglican doctrine ("Letters"). Similarly, during the Exclusion Crisis, which occasioned the writing of *Two Treatises*, the concern was not so much seizure of property as the worry that "the Parties of Men, cram their Tenets down all Men's Throats, whom they can get into their Power" (*Essay* IV, 3, 20).

For Locke this is the true slavery, to which free thought is both antidote and opposite. Seizures of property and involuntary servitude are undoubtedly violations of our liberty. But that liberty is one which takes freedom of thought as its ultimate reference point. The liberty of a Lockean man is found in his mind, not in his property. To be unfree in action means to labor unwillingly on behalf of another. To be unfree in thought, because of Locke's special notion of labor, means largely the same thing. A subjected mind is one that labors in the grips of another's spiritual authority. It forms ideas at the direction of another. Whether one acquiesces in another's beliefs at the point of a sword or out of one's own religious enthusiasm (which Locke calls tyrannizing over one's own mind), it is the most vile and miserable state one can imagine (*Essay* IV, 19, 2). Taking the beliefs of another for one's own does not mean taking up the products of their thought, it means producing those thoughts in oneself by laboring in imitation of another. Ultimately, there is some degree of acquiescence, since we cannot be forced to move our minds in the same ways our body can be forced. Yet, an absolute government can threaten us with death, a death for both mind and body.[14] And the mind can be swayed by this as it is by the temptation of physical pleasure.

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[12] *Slavery* he defines as the absolute, despotical power of one man over another (*Two Treatises*, II, 22–4). Note that Locke, the theorist of property, does *not* consider slaves the property of their master (although he probably thought their products were). His conception of slavery is one of a power relation (in the case of prisoners of a just war, which cannot include civilians) or a matter of obligation (in the matter of one sentenced in a capital crime), not a property relation. Thus, Locke makes no distinction between an economic and a political slavery. Undoubtedly, this hampered his understanding of slavery as a social phenomenon, but it made him acutely sensitive to political domination in a manner similar to the founders.

[13] From time to time it has been the strategy of those seeking to deny Locke's influence to note that his religious and epistemological writings were often preferred to his "political" writings, like *Two Treatises* (Dunn 1969b; for an extreme version of this thesis, see Handlin and Handlin 1989). While I have never found these arguments convincing, it should be noted that, even if true, they prove little. The reader may have noticed by now how many of Locke's ringing endorsements of freedom can be found in that "philosophical" text, *An Essay Concerning Human Understanding*. Surely the distinction between purely political and philosophical texts is a spurious one; even if the colonists had only read the *Essay* (which is certainly not the case), they still might have found grounds for rebellion within it.

In sum, then, Locke's account of the mind's activity as labor forms the basis for his critique of political subjection. The mind's liberty to labor in peace is the source of liberty in action. This liberty, however, does not mean giving full expression to desire but, in fact, restricting desire in the name of virtue. So while Locke's account of human experience may seem especially grim to us (see Mehta 1992), with its constant reliance on the image of a mind at work, it nonetheless provides a spirited defense of individual liberty.

We should also take care to notice the part that Locke's unusual notions of freedom and slavery will play in his account of civil government. Characters who are free most importantly in the realm of ideas may face special sorts of threats. The threat of enslavement may come not only from those who chain the body but also from those who attempt to chain the soul. The Lockean citizen will, it turns out, face just such an attempt on his freedom—at the hands of the king (1960, II, 210). But both the danger to the citizen and the freedom the citizen seeks can be understood only from the viewpoint of the unusual Lockean character whose mental activity is conceived as a form of work. That is to say, Locke's understanding of history cannot be divorced from his ascetic, laborious, Puritan account of man—and of the world.

The Lockean charge that the king was attempting to reduce free men to slavery and rob them of their virtue was echoed in almost identical terms by the American colonists. They, too, I argue below, saw the world as a place in which liberty was endangered by despots who would not allow citizens to labor as they pleased, either materially or mentally. Here, again, we must emphasize that the political use of such terms as *virtue* and *slavery*, which have been regularly taken as evidence of a republican paradigm of thought, have a perfectly intelligible Lockean meaning. What is more, Locke attached these terms and this account of political life to something not found in Aristotle, Machiavelli, Cicero, or Harrington, something in which the colonists had a keen interest—a defense of political revolution. What we will have to determine, when we come to examine the founders' writings, is not so much their specific vocabulary (now that we know that much of the vocabulary taken to be distinctively republican is in fact equally liberal) but the broader character of the political thought within which that vocabulary appears, as well as the use to which it was put. Lockeanism, it has been assumed, would need to be about property and self-interest. But the founding generation, much closer in time than ourselves to Locke, did not share this view. To them, a Lockean politics demanded both personal virtue and an abomination of political slavery. And unlike Machiavelli or Cicero, Locke advised revolution in defense of these principles, advice the colonists accepted.[15]

## THE POLITICAL THEORY OF JEFFERSON AND ADAMS

In order to support this strong claim, I have chosen to examine the political writings of two of the most important founders: Jefferson and Adams. While others might have been chosen (Madison, Hamilton, Washington, Franklin, and Wilson come to mind as the most important), there are good reasons for choosing these two. Unlike all the others named, Jefferson and Adams were constantly involved in American politics (often as leaders) from the 1760s through the 1810s. Furthermore, for much of this period, they were seriously at odds. Adams was president as the candidate of the Federalist Party, while Jefferson was his bitter opponent and successor as head of the Republicans. It is just because of this political antagonism that it suits our purpose to examine jointly the political theory of these two founders. For if we can find some common ground between them, we might then plausibly expect that these commonalities form the basis for a consensus within which the American politics of the period can be said to operate. And if these commonalities appear in the realm of political theory, so much the better. Of course, such a finding could be contradicted by further studies that reveal the views of both men to be anomalous; even so, the agreement of Jefferson and Adams on a wide variety of theoretical issues must, by itself, be an interesting fact since these two contributed so much to our political and constitutional culture. It is also the case that, among the founders, Jefferson and Adams possessed two of the most well-developed political theories. Although many of the others made significant contributions to what we would today call constitutional theory, Jefferson and Adams made the most extensive efforts to reason this theory out from first principles rather than take those principles on faith. As Shain (1994, 14ff.) has pointed out, this sort of theoretical ambition was relatively rare, even among the leading colonial figures.[16]

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[14] Locke makes this point with a play on words: "Some (and those the most) taking things upon trust, misimploy [sic] their power of Assent, by lazily enslaving their Minds, to the Dictates and Dominion of others.... Nor is it a small power it gives one Man over another, to have the Authority to be the Dictator of Principles and Teacher of unquestionable Truths" (*Essay* I, 4, 22–4). Whoever directs one's thoughts is, effectively, one's Dictator.

[15] Alan Houston has pointed out that Algernon Sydney, whose name was often invoked by the colonists, also defended revolutionary action. Houston has done a remarkable job of reconstructing Sydney's thought and showing its parallels with that of the founders. Houston's reconstruction only emphasizes, however, the high degree of similarity between Sydney's theory and that of Locke, even to the point of their similar uses of the rhetoric of freedom and slavery. Two principal points, however, which Houston himself makes, point to the secondary influence of Sydney on the Americans vis-à-vis Locke. First, Sydney was not the systematic thinker that Locke was, and "there is no place in his writings where one can turn to find a 'theory' of liberty"; whereas Locke, as we have just seen, has an extremely well-developed theory. Second, in his examination of the founders' writings, Houston repeatedly finds that it is Sydney the martyr rather than Sydney the author who most inspired the Americans. Detailed references to his writings are remarkably thin, whereas, as we shall see, this is not the case with Locke. See Houston 1991, 115 and chapter 6.

Despite their intense rivalry, both men gave evidence that a common political theory underlay their disputes. Jefferson and Adams opposed one another in the presidential election of 1800, the most acrimonious such contest that had yet taken place. The campaign rhetoric of both sides (to which neither candidate directly contributed, since seeking the presidency openly was frowned upon in that period) depicted the opponent as a virtually treasonous enemy of liberty, bent on destroying the republican experiment. Yet, in the wake of this mean-spirited election (in which the Federalists were trounced), Jefferson was surprisingly magnanimous. In his inaugural address he chose to emphasize the synonymy of his views with those of his adversaries: "Every difference of opinion is not a difference of principle. We have called by different names brethren of the same principle. We are all Republicans, we are all Federalists" (1984, 493).

Eventually, Adams found himself in agreement with Jefferson's thesis. He often displayed this agreement grudgingly, we might say, by way of complaint. Adams liked to depict himself as a prophet without honor in his own land. Some historians have used these occasional moments of self-pity to insinuate that Adams's political theory was at odds with the prevailing consensus. But his complaint was usually the opposite. In 1813, Adams remarked that, although he had helped to define the political theory that had been used to justify independence, he had not received the same degree of credit as Jefferson, whose theories were parallel to his in important areas:

> The declaration of independence of 4 July 1776, contained nothing but the Boston declaration of 1772 [written by James Otis] and the congress declaration of 1774. Such are the caprices of fortune. This [latter] declaration of rights was drawn by the little John Adams. The mighty Jefferson, by the declaration of independence of 4 July, 1776, carried away the glory of the great and the little (1851, 6:278).

While they remained political opponents, both men testified to a certain commonality or, at least, similarity of views. But what was the nature of this overlap? In what follows, I will maintain that Locke's arguments, as outlined above, form a common basis for the political theory of both men. For each of them we can trace out the same pattern observed in Locke: from a moral theory which values labor and self-denial in the fashion of a Protestant Ethic to a political theory that depicts virtue and slavery in terms of this moral theory. Of course, neither man was an automaton who parroted Locke's views. Their relationship with Locke was one of sympathy, not discipleship.

While much evidence could be given on the frequency with which the founders, in general, and Jefferson and Adams, in particular, cited or quoted Locke, I have chosen not to lay too much stress on this element of the argument for two reasons. First, these points have been made extensively by Dworetz (1990), who concludes that Locke was "*by far* the most frequently cited nonbiblical source" within the revolutionary literature (35, emphasis in original). Second, as Dworetz points out, there is a limit to how convincing such evidence is, since the founders might well have cited Locke mistakenly, or with poor knowledge of him, as Bailyn maintains (1967, 28). It will be much more convincing, I believe, to show the similarity of the founders' doctrine, in substance rather than in labeling, to that of Locke.[17]

## The Defects of Rome and the Virtues of Stoicism

Before turning directly to this question, however, it may be useful to consider briefly the attitudes of Jefferson and Adams to ancient Rome and its philosophy, supposed by some to be the touchstone of colonial political thought and the source of the colonists' language of "virtue" (e.g., Elkins and McKitrick 1993, chapter 1). Jefferson, it turns out, was thoroughly disdainful of Rome's political example, while Adams was only slightly more approving. The situation is more complicated in the case of ancient philosophy and throws considerable light on what the founders meant by virtue. Let us consider them in order.

For Roman political history, both republican and imperial, Jefferson never had much use. While he undoubtedly admired many of the cultural achievements of Roman civilization, his description of their government was remarkably one-sided:

> When the enthusiasm, however, kindled by Cicero's pen and principles, subsides into cool reflection, I ask myself, what was that government which the virtues of Cicero were so zealous to restore, and the ambition of Caesar to subvert?

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[16] To me, this last point outweighs the largest drawback to choosing Adams and Jefferson, the fact that neither was able to attend the Constitutional Convention of 1787. None of the works of those present at the convention, it seems to me, have the theoretical ambition of those of Jefferson and Adams. Even Madison's discussions of the fundamental issues of political theory (as opposed to constitutional theory) are remarkably brief, although at least one secondary account of them (Matthews 1995) tends to support my thesis.

[17] Nonetheless, since I do not pursue this point below, it may be useful to mention here that Jefferson, in particular, testified to his own familiarity with all of Locke's major works and to the centrality of Locke's thought for his own: Jefferson 1984, 479 (where he tries to have Locke's political philosophy declared the official philosophy of the University of Virginia), 745, 906, 939–40, 1176; Jefferson 1904, 11:420–4; and Jefferson 1903, 2:253ff., where he makes a long summary of Locke's religious views. The case of Adams is clearly more complicated, but he, too, gives evidence that he had read and agreed with, at the very least, Locke's major works (the *Essay* and *Two Treatises*): Adams 1851, 3:438, 449, 456–7; 4:194, 302; 6:455). At key points in his political writings, Adams quotes Locke directly (1851, 4:83–4). Having said that, it must be admitted that Adams's level of enthusiasm for Locke never reaches that of Jefferson. Nonetheless, it will be argued below that, of the variety of influences which Adams cites, it is that of Locke which is truly predominant in his moral and political philosophy.

> And if Caesar had been as virtuous as he was daring and sagacious, what could he, even in the plenitude of his usurped power, have done to lead his fellow citizens into good government? I do not say to *restore it*, because they never had it, from the rape of the Sabines to the ravages of the Caesars. . . . [The Roman republicans] had no ideas of government themselves, but of their degenerate Senate, nor the people of liberty, but of the factious opposition of their Tribunes. . . . *It has been the fate of that delightful country never to have known, to this day, and through a course of five and twenty hundred years, the history of which we possess, one single day of free and rational government* (1904, 12:152–4, second emphasis added).

Even Cicero, in this passage, hardly escapes condemnation, as Jefferson derides the system to which he lent his services, leaving the philosopher to be considered either as a toady or a fool. Jefferson's condemnation, it should be noted, encompasses not just Rome but all of Italian history, including the period of the Florentine republics that historians have found to be the origin of modern republican theory.

Jefferson's tirade might be written off as unusual except that it actually came in a letter to Adams in response to Adams's own critique of the Romans, rendered in only slightly less forceful terms. Adams reluctantly grants that a few Romans may have been virtuous, but he hardly sees virtue, of any kind, as typical of Roman political culture. Nor does he recommend that period as being in any way a model for Americans. We learn, he thinks, from Rome's failure, rather than its success:

> I never could discover that [the Romans] possessed much virtue, or real liberty. Their Patricians were in general griping usurers, and tyrannical creditors in all ages. Pride, strength, and courage, were all the virtues that composed their national characters; a few of their nobles affecting simplicity, frugality, and piety, perhaps really possessing them, acquired popularity amongst the plebeians, and extended the power and dominions of the republic, and advanced in glory till riches and luxury came in, sat like an incubus on the Republic . . . (in Jefferson 1903, 15:237).

Here, Adams even derides the Roman virtues of "pride, strength, and courage," suggesting that these were insufficient for the support of "real liberty." It is other and rather more Christian, more Lockean, virtues which redeem a few of the Romans, although even here, Adams implies, most of them were fakes. This remarkable exchange ought at least to convince us that, far from being smitten with ancient liberty, the colonists often found Rome lacking in "real liberty" and had no difficulty viewing Roman institutions from a critical distance.

It would be quite wrong, however, to say that Jefferson and Adams took nothing positive away from the many Greek and Latin texts they read. Both men drew a sharp distinction between the ancients' political philosophy and what we might term their moral philosophy, or philosophy of personal conduct. Only the latter was worthy of praise.

> Let a just view be taken of the moral principles inculcated by the most esteemed of the sects of ancient philosophy, or of their individuals; particularly Pythagoras, Socrates, Epicurus, Cicero, Epictetus, Seneca, Antoninus. I. Philosophers. 1. Their precepts related chiefly to ourselves, and the government of those passions which, unrestrained, would disturb our tranquility of mind. In this branch of philosophy they were really great. 2. In developing our duties to others, they were short and defective (Jefferson 1904, 9:460).[18]

Who, exactly, is being praised here and why? Not just any ancient philosophers, and not those ancient philosophers who are particularly known for fostering civic virtue, but specifically philosophers of Stoicism (see Koch 1943, 1–8). It was Stoicism that emphasized the ascetic doctrine that the passions ought to be brought under control by the exercise of reason. This asceticism was then brought into Christian theology through the writings of Augustine, Tertullian, and others. And it was this tradition of Christian asceticism, as we have seen, which fed Locke's doctrines of self-denial and the combat against the passions. Locke listed as the first tenet of *Christianity* that, in order to be virtuous, one ought to make war upon one's own "lusts and vices." In other words, rather than endorsing an ancient philosophy which opposed Locke's liberalism, Jefferson, in praising the Stoics, is praising some of Locke's most important forerunners. When Adams praises the Romans for having taught that "Reason holds the helm, but passions are the gales" (1851, 6:243), he is enunciating a position fully in sympathy with that of Locke. What has obscured this, in recent years, are interpretations of Locke that depict his philosophy as one of selfishness and material accumulation.

As will be described in more detail below, asceticism is at the root of Jefferson's and Adams's moral philosophy, as it is of Locke's. The founders were perfectly capable of distinguishing between the political institutions of ancient Rome and the philosophy of a few of its slaves and citizens. They chose the latter. The colonists' asceticism divides them neither from Christianity, nor from liberalism, nor from Locke. Rather, it was Locke's remarkable ability to combine both of these doctrines with a defense of revolution that rendered him so attractive to the founders. What was defective in Stoic philosophy, they thought, was the manner in which it translated personal asceticism into political principles. It was Locke who found the formula, they felt, which properly linked morals to politics. The declared interest of Jefferson and Adams in ancient philosophy, then, hardly negates their express endorsements of Locke's political theory. If anything, it indicates that they understood Locke better than many twentieth-century commentators. We turn now to that understanding in more detail.

## Virtue and Labor

The interpretation of Locke offered above turned on the special use he makes of the term *labor* and the moral and political consequences that flow from this special use. It was argued that, for Locke, *labor* has a dual meaning. It refers both to physical and mental activity, and both kinds of activity are identified with virtue, so long as this labor is self-directed. When one's labor is not under one's control, one is in a state of slavery. But since labor has several modes, so there are several corresponding sources of the threat of slavery. From within oneself comes the threat of indulgence of the passions at the expense of frugality and industriousness. From outside come the threats of both mental enslavement (through restrictions on liberty) and physical enslavement (through the seizure of property). Jefferson and Adams, I shall argue, both operated within this framework of Lockean virtue and slavery. Where they differed was in which possible Lockean danger they stressed the most. Although each recognized all of Locke's dangers, Adams stressed the first and third ones, Jefferson the second.

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[18] Jefferson says of Aristotle and the Greeks (1903, 15:65): "I think little edification can be obtained from their writings on the subject of government."

***Virtue in Labor.*** Both Jefferson and Adams idealized the hard-working laborer and contrasted him with what they supposed to be his opposite: the slothful or gluttonous person. Adams even went so far as to duplicate Locke's attempt to render this contrast in a single parable. Locke told the tale of an industrious man who through hard work and the denial of any indulgences accumulates a tidy sum, only to see his son, through dissolute habits, quickly waste it away. Whether because he read this passage or merely because he thought along parallel lines, Adams tells virtually the same story:

> See, what is no uncommon sight, a family of six sons. Four of them are prudent, discreet, frugal, and industrious men; the other two are idle and profligate. The father leaves equal portions of his estate to all the six. How long will it be before the two will request the four to purchase their shares? and how long before the purchase money will be spent in sports, gambled away at races, or cards, or dice, or billiards, or dissipated at taverns or worse houses? (1851, 6:459).

In both cases, it is the self-regarding Christian virtues of prudence, frugality, and industriousness that are stressed, rather than the republican ones of courage, virility, or devotion to the polis. As we have seen, the former virtues are not the opposite of liberalism but, instead, can form the foundation for it through the Protestant Ethic. Thus, in the midst of the Revolution, Adams proclaimed that the sort of character he hoped would emerge from it was one "sober, industrious and frugal" (1851, 4:199).[19] We will return to the political implications of this ethic in more detail below. At this point, we are only trying to establish that the founders' views of virtue through labor mirrored that of Locke.

This view was even more pronounced in Jefferson's writings. While the commerce of Boston often lent its flavor to Adams's idea of "industry," Locke's repeated descriptions of labor in terms of land were often echoed by the Virginian:

> Are they afraid they will be reduced to labour the earth for their sustenance? They will be rendered thereby both honester and happier. An industrious farmer occupies a more dignified place in the scale of beings, whether moral or political, than a lazy lounger, valuing himself on his family, too proud to work, & drawing out a miserable existence by eating of that surplus of other men's labour which is the sacred fund of the helpless poor (1984, 589).
>
> Those who labor in the earth are the chosen people of God, if ever he had a chosen people, whose breasts he has made his peculiar deposit for substantial and genuine virtue.... Corruption of morals in the mass of cultivators is a phaenomenon [sic] of which no age nor nation has furnished an example (1984, 290).

Jefferson shared with Locke the model of the single agricultural laborer, working his own plot of land, succeeding or failing by his own devices. Although the lessons drawn from this image were meant to apply broadly, it was this icon to which both inevitably returned when attempting to clarify their views. This figure bears only a faint resemblance to the civic-minded man of republican virtue. The industrious laborer spends a lot more time by himself. He is primarily concerned not with the virtue or common good of his state but with his own. This virtue is cultivated in the same manner as the land—through labor, not through political action.[20]

***The Danger of Passion.*** Both Adams and Jefferson recognized threats to this virtue similar to those outlined by Locke. We can divide those threats into internal and external varieties. The latter will be dealt with below. Of the former, we can say that Adams placed considerably more stress on them than Jefferson. Self-discipline was not a matter on which the Virginian placed great emphasis. Convinced, as he was, that virtue was closely linked to happiness, he did not expect that labor and the avoidance of indulgence would be a burden: "A mind always employed is always happy. This is the true secret, the grand recipe for felicity. The idle are the only wretched. In a world which furnishes so many employments which are useful, and so many which are amusing, it is our own fault if we ever know what ennui is" (1984, 896–7). Jefferson does not disagree with Locke so much as soften his conclusions. Although he agreed with Locke that the choice lay between labor and indulgence, Jefferson found it to be a simple choice; he did not believe that the strenuous education Locke recommended would necessarily be required to achieve the desired state of mind.

It is Adams, in this case, who reproduces Locke's tone most faithfully. Adams's quasi-Puritan railings against the passions match Locke's urgency in fomenting "war against one's own lusts and vices." From an early age, Adams's diaries reflect the concerns of a soldier who feels he is losing such a war.[21] His political theory, too, focuses on the internal threats to self-determination:

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[19] The remark appears in "Thoughts on Government," published in 1776.

[20] Hannah Arendt (1958) drew a sharp distinction between labor and action, wherein the latter was something specifically oriented toward political matters, and the former designated something with a material end. A similar distinction is made by republican theorists between the work of possessive individualism, which results in personal accumulation, and the political work of the virtuous man of republican theory. Indeed, Pocock has gone so far as to call his republicanism, quite anachronistically, "Arendtian."

> Indeed, it must be confessed, and it ought to be with much contrition lamented, that . . . those consciences, which by the commission of God Almighty have a rightful authority over us, [are willingly suffered by us] to be deposed by prejudices, appetites, and passions, which ought to hold a much inferior rank in the intellectual and moral system. Such swarms of passions, avarice and ambition, servility and adulation, hopes, fears, jealousies, envy, revenge, malice, and cruelty, are continually buzzing in the world, and we are so extremely prone to mistake the impulses of these for the dictates of our consciences,—that the greatest genius, united to the best disposition, will find it hard to hearken to the voice of reason, or even to be certain of the purity of his own intentions (1851, 3:435).[22]

The language Adams employs here speaks as much of a political understanding of the mind as it does of the influence of a mental struggle on politics. This is to be expected when a loss of liberty is something that can be experienced both by a polity and by a single laboring mind. Reason's godly "commission" of "authority" is "deposed" by the passions, which then dominate the mind. Nor is this a mere manner of speaking: It follows from the Lockean view that tyranny and slavery are something that may be established over the mind as well as the body. Adams returns to this imagery repeatedly in discussions of the threats to liberty:

> Let us hear the danger of thraldom to our consciences from ignorance, extreme poverty, and dependence, in short from civil and political slavery (1851, 3:462–3).

> It is weakness rather than wickedness, which renders men unfit to be trusted with unlimited power. The passions are all unlimited. . . . If [men] surrender the guidance for any course of time to any one passion, they may depend upon finding it, in the end, a usurping, domineering, cruel tyrant (1851, 4:406–7).

The same language of tyranny that Adams applied to King George is here applied to one's own passions. The similarity results from a Protestant Ethic which equates threats to mental and bodily freedom because both endanger the self-determination of labor.[23] Where Locke was most concerned with the temptations of pleasure, Adams, while registering that concern ("The numbers of men in all ages have preferred ease, slumber and good cheer to liberty, when they have been in competition" [1851, 6:418]), is worried about an even greater range of passions. For Adams, the Lockean virtues of industry and frugality are menaced by a wider set of unruly urges that seek to enslave us for their own purposes. Chief among these cravings, in Adams's mind, is that for esteem. Vanity threatens to enslave us to the opinions of others: "[Men] are really thus constituted by their own vanity, slaves to mankind. Slaves, I say again. For what a folly is it!" (1851, 6:245). Vanity, for Adams, is a cousin to self-indulgence. Like indulgence, it draws us away from our work by promising an effortless pleasure—in this case, the pleasure derived from being admired by others. But, also like indulgence, its true result is slavery, for it is always slavery to abandon one's self-direction for the dictates of another. Locke's concern with the passions, no more than duly noted by Jefferson, finds a more vigorous advocate in Adams.

***The Danger of Dogma.*** The preceding discussion serves to make the following point: The founders' political worries about "slavery" derive from their Protestant understanding of the soul rather than from republicanism. Shain (1994), although he does not mention Locke as a source, has emphasized how widespread this sort of use of *slavery* was in the colonial period throughout what he calls the "political class" (chapter 8). Its theory follows Locke in beginning with an account of human nature that emphasizes virtue in labor and then enumerates dangers to that virtue which proceed, so to speak, from the inside out. That is, rather than viewing liberty as a fundamentally political concept (with slavery as its political opposite), liberty is first and foremost a moral and intellectual concept deriving from an account of human nature, not human politics. Slavery is, for Jefferson and Adams, something that would threaten even a solitary: One can be tyrannized by one's own passions. This is not to say that they were unconcerned with political threats to liberty; on the contrary, these were their primary concern. The point is that such political perils were viewed through the lens of a Lockean political theory which was fundamentally individualist and ascetic, rather than through a civically oriented republicanism.

This link can be further clarified through a discussion of another of these threats, that of dogma. For Locke, as we saw above, freedom of thought was the primary freedom, and "he is certainly the most subjected, the most enslaved, who is so in his Understanding" (*Essay* IV, 20, 6). The founders shared this fear of an imposed orthodoxy. Here, however, it is Jefferson who is most concerned and Adams who is more sanguine. Like Jefferson's analysis of laziness, Adams does not worry much about the dogmatic threats to a free mind, so long as it is in good order: "Liberty, according to my metaphysics, is an intellectual quality; an attribute that belongs not to fate nor chance. . . . The definition of it is a self-determining power in an intellectual agent. It implies thought and choice and power" (1851, 6:448). While Adams, along with many other northerners, worried at the time of the Quebec Act (authorizing Catholicism in that province) about a religious imposition by the king and included provisions for freedom of conscience in his constitution for Massachusetts (1851, 4:221), it was left to Jefferson to make the most forthright statement for freedom of thought in his bill for the establishment of religious freedom:

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[21] Adams 1851, 2:5: "Oh! that I could wear out of my mind every mean and base affectation; conquer my natural pride and self-conceit; expect no more deference from my fellows than I deserve; acquire that meekness and humility which are the sure mark and characters of a great and generous soul; subdue every unworthy passion, and treat all men as I wish to be treated by all. How happy should I then be in the favor and good will of all honest men and the sure prospect of a happy immortality" (diary entry of February 16, 1756).

[22] Adams's use of *conscience* here as something synonymous with reason ("the voice of reason") and that which opposes the passions closely parallels Locke's usage. (See Locke, *Essay* II, 28).

[23] There should be no question about the religious origins of Adams's sketch of the human mind: "Suppose a nation in some distant region should take the Bible for their only law-book, and every member should regulate his conduct by the precepts there exhibited! Every member would be obliged, in conscience, to temperance and frugality and industry; to justice and kindness and charity toward his fellow men; and to piety, love, and reverence, towards Almighty God. In this commonwealth, no man would impair his health by gluttony, drunkenness, or lust; no man would sacrifice his most precious time to cards or any other trifling and mean amusement" (Adams 1946, 5). While Adams's religious fervor wore off with time, his Christianity is still the clear source for his moral views.

> Almighty God hath created the mind free, and manifested his supreme will that free it shall remain by making it altogether insusceptible of restraint; that all attempts to influence it by temporal punishments, or burthens, or by civil incapacitations, tend only to beget habits of hypocrisy and meanness (1984, 346).

Jefferson spoke of the opposite of religious freedom as "religious slavery": "Our rulers can have authority over such natural rights only as we have submitted to them. The rights of conscience we never submitted, we could not submit. We are answerable for them to our God" (1984, 285). When Jefferson learned that some churches were opposing his bid for the presidency on the grounds that he would oppose their establishment, he wrote: "And they believe rightly; for I have sworn upon the altar of God, eternal hostility against every form of tyranny over the mind of man" (1904, 9:148). This carefully phrased remark, which emphasizes the religious roots of Jefferson's tolerance, clearly marks the parallel between his opposition to religious establishment and that of Locke. Both are latitudinarian, that is, their views derive not from an antireligious position but from the radical-Protestant belief that each man should be equally free to think on religious matters for himself. As we have seen in the case of Locke, this view derives from a larger view about the nature of mental liberty in all spheres. It is no different with Jefferson:

> I never submitted the whole system of my opinions to the creed of any party of men whatever in religion, in philosophy, in politics, or in anything else where I was capable of thinking for myself. Such an addiction is the last degradation of a free and moral agent. If I could not go to heaven but with a party, I would not go there at all (1984, 941).

For Jefferson, as for Locke, dogmatic thinking is not just a matter of intellectual laziness (although it is that); it is "the last degradation of a free and moral agent." This might seem odd from a republican perspective, which places more weight on freedom to participate and act than on freedom of conscience, which is the freedom of a solitary. It makes perfect sense, however, from the standpoint of an ascetic liberalism, wherein the freedom to labor, even in private thought, is the primary freedom. The threat of a royal orthodoxy moved Locke to write his "Letters Concerning Toleration"; a similar danger occasioned Jefferson's legislation on religious freedom. As with the danger of passion, the danger of dogma recognized by the founders is best understood by reference to a Lockean theoretical framework. It was the Lockean understanding of freedom as something that begins with the solitary mind that grounded their political opposition to overweening state power. In the question of the mind's relation to both freedom and slavery, then, the founders' philosophy shows every sign of a deep sympathy with that of Locke.

## CONCLUSION

Jefferson's opinion that between Adams and himself there was no "difference of principle" has been borne out by our attempt to situate both of their theories in relation to that of Locke. The degree to which either man absorbed his views from Locke directly (although there is much evidence for this) is much less important than the fact that Locke elaborated a complex theoretical framework with which both Jefferson and Adams can be shown to have been in considerable sympathy. All three had similar accounts of virtue, rooted in the Protestant Ethic, and all three built on this account a system of politics based on the freedom to labor, both in mind and in body. It was a Lockean slavery the Americans feared and a Lockean virtue they sought.

We have seen, too, that both Jefferson and Adams declared this understanding of government to be not just their personal position but the broader understanding of the revolutionary generation. While we should not take such assertions on faith, neither should we dismiss them out of hand. In addition to their testimony, we have the fact that they agreed on so much even while standing at opposite ends of the political spectrum of the early republic. Whoever stood apart from their agreements, then, would have had to disavow *both* the leading "liberal" and the leading "conservative" of that day.

I have not attempted here a complete analysis of the political theory of Jefferson and Adams or of the founders generally. Such an effort would undoubtedly have to take note of the extensive disagreements within this generation and between these two men. My concern, instead, was to investigate the old theory of Lockean consensus, in light of the attempts made to replace this account, and to see of what use it still might be. One point, in particular, should be clarified: My interpretation is not meant to contradict those readings of Locke (and the founders' Lockeanism) which stress the elements of property and social contract. My point is only that these elements have a prior theoretical ground in a notion of liberty and slavery of the mind. Despite the wealth of evidence that historians have uncovered on the subject of republicanism, I have found that terming the founders' theory "Lockean" still makes the most sense. However inspired some (clearly not Jefferson or Adams) may have been by Roman imagery, the colonists' fundamental political theory was Lockean through and through.[24]

By paying closer attention to what links Locke to the founding generation, however, we see just as clearly what separates that generation from our own. The principal difference is the Christian foundation for moral and political theory which the founders assume. What was, for them, an unproblematic premise is, today, probably a minority position and certainly a highly controversial one. Modern liberalism, then, bears at best a family resemblence to the Lockeanism of the framers of the Constitution, not merely in its secular orientation but also in its emphasis on the idea that political principles ought to rest on widely shared, uncontroversial values (Rawls 1993). It may be that modern liberals have no more business than republicans in resting their claims on an implied link between their theories and early American political culture. That a Lockean sympathy once existed across a wide range of American political thinkers does not mean that such a situation obtains today; nor does it mean (and this should be emphasized) that, if such a situation did obtain, it would be a good thing. Especially in a moment when religious politics are experiencing a resurgence, establishing the founders' Lockeanism must surely raise as many questions as it settles, and not simply historiographic ones.

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[24] I explore the issue of social contract more thoroughly in "Between History and Nature: Social Contract Theory in Locke and the Founders," *Journal of Politics*, forthcoming.

As comfortable as it may make us to reason as if there exists an abiding dominant voice in American political and constitutional thought, such a conclusion cannot easily be supported. The departures from the theories of the founders have probably been more important than the continuities. The founders' political thought is unimaginable without their Puritanism; while we retain many traces of that creed, we do not embrace it as readily as they. We ought to recognize, then, the degree to which we remain politically and culturally indebted to a variety of political thought with which we often do not agree. Yet, we still inhabit political and constitutional structures that bear substantial similarity to those of the late 18th century. If there are elements of the founders' theory, hence of the American soul, with which we are uncomfortable, then we must come to terms with that. But before we either praise or condemn the founders' Lockean sympathy, we must recognize it for what it is. Or else, as Hartz might say, we will be prone to the neuroses that always result from repressing an element of our collective psyche.

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# Assessing the Dyadic Nature of the Democratic Peace, 1918–88

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*The literature on the democratic peace has emerged from two empirical claims: (1) Democracies are unlikely to conflict with one another, and (2) democracies are as prone to conflict with nondemocracies as nondemocracies are with one another. Together these assertions imply that the democratic peace is a dyadic phenomenon. There is strong support for the first observation, but much recent scholarship contravenes the second. This paper assesses whether the democratic peace is a purely dyadic, a monadic, or perhaps a mixed dyadic and monadic effect. Our analysis offers two important advances. First, our model directly compares the dyadic and monadic explanations by using the state as the unit of analysis rather than the potentially problematic dyad. Second, our model controls for an important but overlooked confounding variable: satisfaction with the status quo. Our results indicate that the initiation of violence within crises is predominantly a dyadic phenomenon, but we also find evidence suggesting a strong monadic effect regarding the emergence of crises.*

The prevailing realist model of international conflict (Waltz 1979, Mearsheimer 1994/5) has come under increasingly severe attack by those who claim that domestic politics has an important influence even on issues which directly concern the "national interest," such as the outbreak of war. The relevance of domestic politics to understanding international conflict is forcefully presented in the rapidly developing body of scholarly work labeled the "democratic peace" literature (e.g., Ray 1995, Russett 1993). A common belief among scholars is that this literature posits two empirical observations about the conflict behavior of democracies: (1) They are unlikely to conflict with one another, and (2) they are as prone to conflict with nondemocracies as nondemocracies are with one another. Together, this is a vision of the democratic peace as a dyadic effect, which implies that democracies are only more peaceful with one another, and not a monadic effect, which would mean that democracies are more peaceful in their relations with all countries.

Although the empirical literature offers strong support for the first observation, the second has in recent years come under increasing attack (Ray 1995). Much of the most recent empirical scholarship contravenes the second point, indicating instead that democracies are unlikely to conflict with authoritarian as well as democratic states. Flaws in the research designs of these recent studies, however, casts doubt on their challenge of the second proposition.

This paper seeks to remedy those flaws and assess whether the democratic peace is a purely dyadic effect, a monadic effect, or perhaps a mixed dyadic and monadic effect (that is, democracies are more peaceful than nondemocracies toward all nations, but especially toward democracies). Our tests of the democratic peace offer two important advances over previous scholarship. First, our model permits a direct comparative test of the dyadic and monadic explanations by using the state as the unit of analysis rather than the potentially problematic dyad. Second, our model includes an important control: We identify whether a crisis participant is challenging the status quo. This is important, because if democratic participants in crises are less likely to have challenged the status quo, and if status quo challengers are in general more likely to resort to violence during a crisis, then the failure to include this control variable can distort our understanding of the causal processes behind the democratic peace by conflating the effect of democracy on the *emergence* of crises with its effect on the *escalation* of crises.[1]

The remainder of this paper consists of five parts. First, we review alternative theoretical arguments for the democratic peace. Second, we assess the existing empirical literature. Third, we present the hypotheses to be tested and their operationalizations. Fourth, we examine empirical results based on the behavior of states in international crises from 1918 to 1988. The results indicate that the initiation and escalation of violence within crises is predominately a dyadic phenomenon. Our findings also suggest, however, that there is potentially a strong monadic effect regarding the emergence of a crisis: Democracies are less likely than nondemocra-

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[1] In this paper international *disputes* are defined as political conflicts between two independent states. If either state threatens to use or actually uses military force, the dispute becomes an international *crisis*. Our primary focus is on the influence of domestic political systems on decisions to use military force during international crises.

cies to initiate crises against all states. The final section summarizes the findings and offers directions for future research.

## THEORETICAL FOUNDATIONS OF THE DEMOCRATIC PEACE

Theoretical scholarship on the democratic peace is characterized by two divisions: the source of the peace and the pattern of interaction. In terms of the source of the peace, most arguments fall into one of two schools: political norms or institutional structures. The normative explanation emphasizes the socialization of political leaders within their domestic political environment (Dixon 1993, 1994; Maoz and Russett 1993; Raymond 1994; Russett 1993). This argument posits that democratic political systems are characterized by compromise and nonviolent resolution of political conflicts. Democratic leaders are socialized into accepting these norms, and as a result the foreign policies of democracies are likely to be nonviolent and reflect the spirit of compromise. Nondemocratic political leaders, in contrast, are socialized in an environment in which coercion and violence are more widely accepted as legitimate means for resolving political disputes. Their foreign policies are assumed to reflect these norms, and hence nondemocracies are hypothesized to be more likely to initiate violence and less likely to compromise.[2]

In terms of the pattern of interaction, the institutional structure school focuses on the relationship between political structures and the domestic political costs of using force (Morgan and Schwebach 1992, Russett 1993). According to this school, decisions to use military force are based on domestic and international cost-benefit calculations. Foreign policy decisions can have costly domestic political repercussions. The expenditure of resources and loss of human life can mobilize opposition groups or fracture a ruling coalition. Relative to other political systems, democratic decision makers must be more sensitive to these potential domestic costs. This constrains their behavior in comparison with leaders of nondemocratic states (Bueno de Mesquita and Lalman 1992, Kant [1795] 1971, Russett 1993).

The assumptions that support these two explanations are worth laying out in greater detail. In the remainder of this section we present a series of propositions which reflect what we believe are the underlying assumptions of the theoretical writings on the democratic peace and specify how the ideas of different scholars converge and diverge.

The structural explanation posits four key assumptions.

STRUCTURAL ASSUMPTION ONE. *A central goal of state leaders is to retain their position of domestic political power.*

STRUCTURAL ASSUMPTION TWO. *In all political systems, domestic political opponents of a regime will attempt to mobilize political opposition when domestic and foreign policies pursued by the regime have failed to achieve stated policy goals.*

STRUCTURAL ASSUMPTION THREE. *In democratic political systems, however, counter-elites are better able to mobilize opposition in order to challenge incumbents for their policy failures.*

STRUCTURAL ASSUMPTION FOUR. *In all political systems, state leaders believe that a foreign policy setback for their country, stemming from a diplomatic retreat or military defeat, could pose a threat to their domestic political position.*[3]

In all countries, the ruling regime relies on a supporting constituency to remain in power. In addition, all state leaders must confront political opposition. While the scope and intensity of this opposition varies from regime to regime, political opposition eager to exploit policy failures exists in all states. The more democratic the regime, the higher is the probability that opposition groups will have the opportunity to mobilize opposition and/or splinter the ruling regime's supporting constituency. Democratic leaders, therefore, face a higher expected cost for failure because the probability that the costs are imposed is higher. Decisions to use military force are particularly risky since the likelihood of success and the costs to be incurred are often difficult to predict with high confidence. As a result, foreign policy leaders who face credible political opposition should be more concerned with protecting themselves from a political backlash by avoiding risky military confrontations.

The normative explanation posits two quite different assumptions.

NORMATIVE ASSUMPTION ONE. *Leaders socialized within democratic political systems are more likely to use compromise and nonviolent means to resolve disputes than are leaders socialized in authoritarian political systems.*

NORMATIVE ASSUMPTION TWO. *Norms and conflict resolution practices employed by political leaders when they are involved in domestic disputes are also used when these leaders seek to resolve international disputes and crises.*[4]

Domestic political systems induce political elites to resolve conflict in particular ways. Some political systems promote the use/threat of violence and the imposition of decisions to settle conflict, whereas other systems delegitimize violence and favor compromise to resolve disputes. Democratic systems, by prohibiting the use of threats and violence, socialize leaders in a system built on compromise and negotiation. In contrast, autocratic leaders are socialized in a system which tolerates or encourages the use of force against political opposition; the vast majority of autocratic leaders either rise to power through the use of force or consolidate power by the systematic elimination of actual and potential opposition. Finally, normative explanations assume that regardless of the particular type of norms held by decision makers, they externalize these norms when facing international opponents.

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[2] Within the broad category of norms of compromise and nonviolence, we included an inclination for negotiated outcomes, a favorable attitude toward mediation by third parties, and a willingness to participate in binding arbitration.

[3] Empirical support for the relationship between failure in war and the loss of political power for wartime leaders is presented in Bueno de Mesquita and Siverson 1995 and Bueno de Mesquita, Siverson, and Woller 1992. Also see Russett and Graham 1989.

[4] Several studies of foreign policy in the former Soviet Union have examined the link between internal patterns of political competition among elites and foreign policy behavior. See Goldgeier 1994, Ritcher 1994, and Roeder 1984.

The second theoretical division in this literature is over the causal pattern of the democratic peace; that is, whether democracies are more pacific in their actions toward all other states or only in their actions toward other democracies. We call the former claim the monadic proposition, as it assumes that the democratic peace emerges from the individual behavior of democratic states. The latter argument states that democracies are only more pacific toward one another, which we call the dyadic proposition, as it assumes that pacific policies are adopted in the context of democratic states interacting with one another.

In comparing the monadic and dyadic propositions, it is crucial to distinguish between the use of force in response to force (such as defending against an invasion) and the initiation of force (such as launching an invasion). Consider three kinds of foreign policies. First, a state could be a pure pacifist, unwilling to initiate or use force even in reply to the use of force. Such states are unlikely to remain independent in an anarchic system. Second, a state could be willing to respond with force but less likely to initiate the use of force. This strategy conforms with the assumptions laid out above for both the structural and normative explanations. The structural constraints on the use of force are likely to recede if force has already been used; public opinion may be averse to using force but recognizes the necessity of it when faced with an attack. The norms against the use of force also would not constrain force as a reply to force. For example, within a democracy, police force is sanctioned as a legitimate response to criminal force. A foreign policy informed by this norm then would permit the use of force in reply to the prior use of force. The third kind of foreign policy permits both the responsive use of force and the initiation of the use of force. This is not consistent with the monadic normative or structural explanations, as force is seen as a legitimate tool of foreign policy, not as a last resort. Note that while the second and third types of foreign policy exhibit very different patterns in the use of force, both may be expected to engage in violent behavior with any kind of state under certain circumstances. The critical distinction between these patterns lies in identifying which party initiates force. Thus, in testing arguments about the democratic peace, it is critical that one focus on the initiation of force, not simply the use of force or involvement in wars. Moreover, when examining the escalation of crises one needs to look at the level of military provocation each state faces.

The normative and structural arguments as laid out in the first six assumptions both point to the prediction that democracies are more pacific in their relations with all other states, not just other democracies. That is, these assumptions lead to the monadic prediction that democracies will reciprocate but not initiate force when dealing with all types of states. The structural assumptions imply that democratic leaders should expect a higher potential domestic cost of failure because the political opposition has a greater capacity to punish decision makers. Regardless of the regime type of the adversary, the higher cost of failure should make democratic states less likely to initiate and escalate. Similarly, the two normative assumptions imply that democratic decision makers should emphasize compromise and negotiation regardless of the regime type of the opponent. Reliance on such strategies should significantly lower the probability that democratic states are the first to resort to violence to resolve political disputes.

MONADIC HYPOTHESIS. *In a crisis, the more democratic a state, the less likely it is to initiate the use of force regardless of the political regime of the adversary.*

Some scholars have argued, however, that when democracies face nondemocracies they exhibit the third type of foreign policy, that is, democracies are willing both to reply with force and initiate the use of force against nondemocracies (for the structural explanation, see Bueno de Mesquita and Lalman 1992, 158, and Russett 1993, 40; for the normative explanation, see Morgan and Schwebach 1992, 307). There are at least two reasons a democracy may initiate force when facing a nondemocratic adversary. First, if a democracy believes war is inevitable it may attack first to seize a military advantage (Bueno de Mesquita and Lalman 1992, 158). Second, other theorists have predicted that democracies may use force when they feel the negotiating process has broken down, as "democracies may initiate large-scale violence with nondemocracies rather than make the greater concessions demanded" (Russett 1993, 40). This reasoning predicts that a democracy is less likely to use force against another democracy in such a context, both because negotiations between democracies are more likely to yield a compromise solution and because a democracy is more likely to have faith in negotiations when facing a democracy (Dixon 1994).

The dyadic explanations, therefore, rest on the additional assumption that democratic decision makers fear exploitation. These leaders believe that their nondemocratic opponents view democratic political institutions as a source of weakness in the conduct of foreign policy. The public's aversion to paying the costs of war, the existence of political opposition within and outside the government, and a willingness to compromise all encourage autocratic leaders to challenge democratic polities. The fear of exploitation leads democratic leaders to alter their bargaining strategies (Bueno de Mesquita and Lalman 1992) and/or circumvent institutional constraints (Maoz and Russett 1993) when they are facing a nondemocratic adversary.

DYADIC ASSUMPTION. *The expectation that nondemocratic opponents will often resort to force and/or will refuse to negotiate in good faith leads democratic decision makers to adopt more coercive foreign policies, which include the first use of force in international crises.*

The additional assumption leads one to predict an alternative hypothesis.

Dyadic Hypothesis. *In a crisis, the more democratic a state, the less likely it is to initiate the use of force against other democracies.*

Proponents of the monadic hypothesis reject this line of argument because they question the logical basis for assuming that democratic leaders believe they are exploitable. Democratic norms are compatible with a strategy of reciprocity in bargaining, and such a strategy does not encourage exploitation; it allows states to prepare adequately to defend their interests and to respond with force if attacked. While such a strategy may encourage the offering of contingent concessions, there is no reason to expect leaders following the strategy to make unilateral concessions. If democratic leaders do not believe they are exploitable, the rationale for becoming more coercive and intransigent when facing an autocratic opponent breaks down.

In summary, in contrast to some previous analyses, we have demonstrated that one can use both the normative and structural models to predict either a monadic or dyadic democratic peace.[5] The remainder of the paper addresses only the second of these two theoretical divisions. Our analysis is agnostic about whether the normative or structural model provides a better explanation of the democratic peace. Instead, we focus on the causal pattern, asking the question: Is the democratic peace monadic, meaning that democracies are unlikely to initiate violence against either democracies or nondemocracies, or is it dyadic, meaning that democracies are unlikely to initiate violence against democracies but willing to initiate against nondemocracies?

## THE EMPIRICAL LITERATURE

The empirical literature on the democratic peace provides strong support for the proposition that democracies are pacific in their relations with one another, up to and including war, even controlling for alternative explanations such as geography and alliance ties (Bremer 1993; Bueno de Mesquita and Lalman 1992, 152; Doyle 1986; Maoz and Abdolali 1989; Maoz and Russett 1993; Owen 1994; Russett 1993). The empirical support for the proposition that democracies are as belligerent in their relations with nondemocracies as are nondemocracies with one another is far more mixed. Small and Singer (1976) noted that democracies participated in war just as frequently as autocracies between 1816 and 1976. This was a useful preliminary finding, but just measuring frequency of participation is not a complete answer to the question of war-proneness, as one needs to place the frequency of participation within the context of a larger, systematic sample which includes nonparticipation.

A number of studies have moved beyond the Small and Singer approach by using the summed dyad as the unit of analysis.[6] In doing so they used improved sampling techniques by including cases of peace as well as war, enabling better assessments of frequency. It was hypothesized that if democracies are pacific toward one another but as equally war-prone toward nondemocracies as are nondemocracies toward one another, then we would expect democracy/democracy dyads to be less prone to conflict than all other kinds of dyads. Summed dyad studies offer empirical support for this proposition but also indicate that democracy/nondemocracy dyads are less prone to conflict than are nondemocracy/nondemocracy dyads. In wars since 1816, Bremer (1992) found that war was less likely in democracy/democracy dyads than in democracy/nondemocracy dyads and was less likely in the latter than in nondemocracy/nondemocracy dyads. Hewitt and Wilkenfeld (1995) uncovered similar results. Controlling for whether a crisis was ongoing, the gravity of the issues at stake in the crisis, and the level of violence in the trigger to the dispute, these authors found that as more democracies become involved in a crisis the likelihood that violence will be used in that crisis declines. Two other studies found that crises between nondemocracies were more likely to escalate to war than were crises between a democracy and a nondemocracy, which were in turn more likely to escalate to war than were crises between democracies (Bueno de Mesquita and Lalman 1992, 152–3; Morgan and Schwebach 1992). Maoz and Abdolali (1989) claimed to find that democracies are not generally more pacific, but they did find some limited evidence (p. 18) that democracies are more pacific toward all states. In contrast, one study that focused on a slightly different dependent variable—peaceful settlements—found that while such settlements are more likely when both disputants are democracies, the chances of peaceful settlement when only one of the disputants is democratic is about as likely as if both are not democratic (Dixon 1994; for a somewhat dissenting view, see Hewitt and Wilkenfeld 1995).

Measuring the conflict-proneness of dyads is an inefficient way to assess the pacifism of democracies, as a democracy can be peaceful and yet still belong to several war-prone dyads because it is attacked so often. A summed dyad unit of analysis obscures which state in the dispute is in fact forcing the crisis up the escalation ladder. In the twentieth century, the Belgium-Netherlands dyad has been peaceful, but the Belgium-Germany dyad has been war prone. This is not because democratic Belgium was more war prone in its relations with authoritarian Germany than was democratic Netherlands; Belgium did not initiate violence against either but was invaded twice by Germany (see Chan 1984, 636n). The summed dyad masks this kind of relationship.

The appropriate unit of analysis for assessing whether the democratic peace is monadic or dyadic is the nation-state. Such an approach enables us to discern which states initiate and escalate conflicts, and it thereby better permits the assessment of which states have a pacific foreign policy.[7] A few studies have tried to assess the pacifism of democracies using the nation-state as the unit of analysis. Using frequency counts, Small and Singer (1976, 64–6) found that democracies often have been on the initiating side of wars since 1816, but the study suffered from the previously mentioned sampling problem. Chan (1984, 636–9) conducted a somewhat more sophisticated statistical test and found that democracies were not statistically significantly less likely to be on the initiating side than were nondemocracies, although Domke (1988, 102) found democracies to be less likely to initiate wars of conquest. Leng (1993, 28–9) found that democracies were more likely than nondemocracies to use reciprocating bargaining strategies in international crises, reciprocation perhaps indicating a peaceful state willing to defend its interests. Two studies found that regimes with greater institutionalized constraints on state leadership (a common characteristic of democracies) were less prone to violent behavior than were less constrained regimes (Carment and James 1995; Geller 1985, 181). Schweller (1992) found that since 1665 no declining democratic great power has launched a preventive war against either a rising democratic or nondemocratic great power. Snyder and Mansfield (1995) used the nation as the unit of analysis in their study of democratization and war, but they looked at war participation rather than war initiation, thereby risking the same kinds of biases inherent in the summed dyad unit of analysis. Very few studies (exceptions include Leeds and Davis 1995 and Ray and Wang 1995) have combined all the necessary aspects of a proper test: a data set which includes cases of violence and nonviolence, identification of the state that initiates violence, and explicit comparison of the dyadic and monadic explanations.

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[5] A common misconception is the belief that structural explanations must yield monadic predictions and normative explanations must yield dyadic predictions.

[6] Most empirical tests of the democratic peace have relied on what Kegley and Skinner (1976) refer to as a summed dyad hypothesis. In order to test a summed dyad hypothesis, data are collected at the dyad level (e.g., "does the dyad escalate to war?"). Kegley and Skinner question the utility of summed dyad hypotheses.

Moreover, while a number of the later studies have expanded the domain from the set of very rare interstate wars to the set of relatively common militarized crises, a danger in generalizing the findings still exists. Examining the crisis behavior of democracies amounts to asking whether democracies are less likely to take violent actions in a crisis (or escalate it to war) than are nondemocracies. Yet, the theoretical foundations of the democratic peace should apply to precrisis behavior as well, since we would expect that democracies are less likely to become challengers in international disputes. We also might expect that challengers of the status quo would be more likely to use force first because altering the status quo would generally require action to force the defending state to change its behavior absent capitulation. Therefore, by failing to control for which state is challenging the status quo, one might falsely interpret a precrisis monadic effect of democracies being less likely to challenge the status quo as an intracrisis monadic effect of democracies being less likely to use force within crises. In other words, such analyses suffer the biases of an undetected selection effect (on selection effects in the deterrence debate, see Fearon 1994 and n.d.; see Achen 1986 on selection effects in general).

While the preceding discussion has focused on the debate over the pattern of interaction leading to the democratic peace, we should emphasize that adherents to the realist school reject the existence of either a monadic or dyadic peace. Realists have long argued that the distribution of military power among states and the degree of shared security interests between states are far more important determinants of state behavior than are domestic institutional structures. Realists assume that states resemble unitary rational actors in pursuit of a single overriding objective: survival and security in an anarchic system. The structural features of the international system lead all states to behave in a similar fashion regardless of their particular political institutions, economic structure, or ideological orientation (Waltz 1979).

The rapid growth of the democratic peace literature has, as should be expected, triggered a spirited realist counterattack. Spiro (1994) argues that the apparent peace among democracies is a statistical artifact resulting from the fact that both wars and democracies are rare. He concludes that a more full-bodied realist theory, which incorporates an element addressing the intersubjective understanding of state interests, could explain both alliance and conflict behavior. The inappropriate use of probability theory, however, severely undermines his conclusions (see Russett 1995). Layne (1994) examines four democracy-democracy crises and concludes that realist factors rather than institutional constraints or shared norms prevented the crises from escalating to war. While the use of case studies is an important step in the effort to understand fully the roots of the democratic peace, the examination of a very limited number of cases (the historical interpretations of which have been contested by Owen 1994) cannot decisively disprove a probabilistic argument.[8] Moreover, the fact that three of the four cases examined by Layne occurred during the 1800s raises uncertainty as to the generalizability of his results to the modern era, in which publics and legislatures have a much stronger influence on public policy. Finally, Farber and Gowa (1995; see also Cohen 1995) claim that the democratic peace is epiphenomenal and in reality is a manifestation of common security interests; the lack of conflict among democracies in the postwar period is simply due to the fact that they are much more likely to ally with one another, and alliance partners are unlikely to become engaged in conflict due to shared interests. We investigate this claim in the data analysis section.

This paper contributes to the existing empirical literature on the democratic peace in two important ways. First, it directly compares monadic and directed dyadic hypotheses by using the state as the unit of analysis, initiation as the dependent variable, and a statistical model which permits the separation of dyadic and monadic effects. Second, it controls for which crisis participant challenged the status quo, avoiding the potential bias incurred by conflating precrisis and intracrisis behavior. We will address the important issue of selection effects in the concluding section.

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[7] A nation-state data set contains separate codings for the behavior of each actor in the crisis (e.g., does country A escalate the crisis? does country B escalate the crisis?). In order to test either a "directed dyad" or a "monadic" hypothesis, in the terminology of Kegley and Skinner (1976), requires the use of a nation-state data set. In this paper we test a directed dyadic hypothesis using nation-state data and an interactive term. For simplicity, however, we refer to our test as the "dyadic hypothesis."

[8] While there are exceptions (Rummel 1983), most researchers have proposed and tested probabilistic arguments (i.e., norms and structures reduce the probability of using military force) rather than deterministic laws (i.e., democracies will never use force against another democracy). While selecting on the dependent variable in order to refute a deterministically necessary condition (i.e., a law) is a useful approach, the method cannot be used to refute decisively a probabilistic argument.

## HYPOTHESES, RESEARCH DESIGN, AND MEASUREMENT OF VARIABLES

We now turn to the list of specific hypotheses that we will examine. The logic behind the first two hypotheses has already been discussed in detail.

HYPOTHESIS 1 (Monadic). *In a crisis, the more democratic a state, the less likely it is to initiate the use of force regardless of the political regime of the adversary.*

HYPOTHESIS 2 (Dyadic). *In a crisis, the more democratic a state, the less likely it is to initiate the use of force against other democracies.*

An important component of the dyadic explanation is expressed in the dyadic assumption: Democracies may preemptively initiate the use of force against authoritarian states because they fear exploitation. The argument posits that authoritarian leaders believe that democracies are more likely to capitulate; this belief leads the authoritarian leaders to attempt to exploit democracies by attacking first (Bueno de Mesquita and Lalman 1992, 155–60).[9] If this is true, then we would expect nondemocratic states to be more likely to initiate the use of force against democracies. While we are not claiming to test the dyadic assumption directly, we can probe the underlying logic of the argument with this auxiliary hypothesis.[10]

HYPOTHESIS 3. *In a crisis, nondemocracies are more likely to initiate the use of force against democracies than they are against nondemocracies.*

In addition to our analysis of the democratic peace, however, it is important that we include some controls in our analysis. One important control variable already discussed is whether the crisis actor in question is challenging the status quo. We would expect such actors to be more likely to initiate the use of force because it is often necessary in order to achieve their goals if their opponent does not capitulate. Status quo powers, in contrast, generally need not use force to achieve their goals unless their opponent does so first.

HYPOTHESIS 4. *In a crisis, if the actor is challenging the status quo, then the actor is more likely to initiate the use of force.*

Second, a prominent explanation for the use of force in international crises is the balance of military capabilities. Realists argue that the distribution of power among states should be the most powerful predictor of state behavior. States that enjoy a military advantage should be more likely to initiate force both because their military action is likely to be successful and because the cost is likely to be lower than when one faces a military disadvantage. Weaker states, however, will view the initiation of violence as a very risky strategy which is likely to result in substantial costs.

HYPOTHESIS 5. *In a crisis, the more the balance of military forces tends to favor the actor, the more likely is the actor to initiate the use of force.*

Finally, states that share substantial interests beyond the particular issue at stake in an international crisis may be deterred from escalating that crisis because they fear losing the other benefits they gain from the relationship with their opponent. Realists stress that shared security interests rather than domestic structures or shared norms should dictate decisions to use force. While nonmilitary factors such as trade or cultural ties may affect military action, realists claim that common security interests are the most likely candidates for influencing the escalation of international crises. Perhaps the best observable indicators of shared security interests are military alliance ties. Thus, we predict that allied states will be less likely to initiate force against one another in a crisis (Bremer 1992).

HYPOTHESIS 6. *In a crisis, if the actor has any military alliance ties with its adversary, then the actor is less likely to initiate the use of force.*

We test our hypotheses using the revised International Crisis Behavior (ICB) data set. It contains the population of international crises from 1918 to 1988 in which one or both parties threatened to use, or actually used, military force to resolve a political conflict. The data set represents the most comprehensive attempt to identify the universe of international crises during the interwar and postwar periods. Moreover, the ICB provides much more extensive documentation than alternative data sets that identify the population of militarized disputes.[11]

Our data set is an adaptation of the revised ICB data set. From the ICB's 390 crises, we eliminated six categories. First, we removed 27 crises that occurred within the context of a full-scale war because conceptually we consider decisions to escalate the geographic scope or intensity of a major military conflict as fundamentally different from decisions to initiate an armed conflict or to escalate an existing dispute up to the large-scale use of force. For example, we removed the Stalingrad crisis of 1942 because the German decision to use force in the battle over Stalingrad is fundamentally different from the decision to launch the attack against the Soviet Union (Barbarossa Crisis) in 1941. Once large-scale armed conflict has occurred, we believe that the further use of force becomes largely a matter of military strategy and tactics, while domestic political considerations become much less important.

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[9] Bueno de Mesquita and Lalman (1992) have an existence proof which identifies the possibility of defensive preemption by democratic states in this situation. Reiter (1995), however, finds that preemptive strikes rarely happen.

[10] If democratic leaders can be shown to fear exploitation, even if no empirical evidence exists to support this belief, then the logic of the dyadic argument remains intact.

[11] A description of the ICB's structure as well as summaries of the international crises included in the original version can be found in Brecher, Wilkenfeld, and Moser (1988). The revised version, which uses the same coding rules but adds crises from 1918–29 and 1980–88, is available from the Inter-university Consortium for Political and Social Research (ICPSR 9286) at the University of Michigan. The well-documented data set has been used in a wide variety of international relations studies, including Maoz and Russett (1993) and Hewitt and Wilkenfeld (1995). Unfortunately, no broader data set comprising the universe of international disputes for the period under consideration currently exists. The authors are in the process of creating one. Therefore, the results presented here focus on the role of domestic institutions *within* crises; we cannot generalize to the broader set of international disputes at this time.

Second, we eliminated 39 crises in which, upon investigation, we failed to find evidence of either verbal threats or threats of force accompanied by military deployments by the challenger. Thus, in some cases we could not find evidence in the historical record indicating that the challenger was considering the use force even though the opponent in the crisis perceived a threat (e.g., the Bulgaria-Turkey Crisis of 1925); in other cases the challenger was intervening on the side of the internationally recognized government (e.g., Soviet actions during the East Berlin Uprising Crisis in 1953).

Third, we removed nine crises in which states capitulated to the demands of the challenger prior to the use of force by either side. Challengers in these crises never faced a discrete decision of whether to use force to resolve the political conflict. For example, in the Polish Ultimatum crisis of 1938, Lithuania capitulated to Polish demands for the normalization of relations and the opening of the border between the two countries; thus, Poland did not face a decision of whether to use force in an attempt to coerce a determined adversary.

Fourth, we aggregated 38 crises in which several conflicts erupted within the same basic dispute over a prolonged period. For example, the numerous cross-border raids involving South West Africa People's Organization (SWAPO) guerrillas and South African Defense Forces from 1978 to 1987 were aggregated into a single case. After the initial crisis in 1978, all subsequent crises were treated as "intrawar" disputes because SWAPO guerrilla activity was almost continuous throughout this period.

Fifth, we removed five crises that involved a colonial power versus an indigenous independence movement. Our theoretical framework applies to the bargaining norms and political structures of independent state actors, and independence movements typically lack formal political institutional structures. In addition, it is unclear just what sort of conflict resolution norms are established under colonial rule. For these reasons, we removed colonial cases such as the Indonesia Independence crises of 1945, 1947, and 1948.

Finally, we removed three crises because one or both actors were not internationally recognized as sovereign states as defined by Small and Singer (1982).

We then added several crises by disaggregating conflicts with multiple conflictual dyads. For example, the single case identified by the ICB data set as the Invasion of Scandinavia was disaggregated into two cases: Germany versus Denmark and Germany versus Norway. After removing and disaggregating crises, we arrived at a final data set of 303 conflict dyads occurring within 272 international crises. For each crisis, we collect data on both the challenger and defender states because political leaders in both countries must decide when and to what extent military force should be used.[12] As a result, the data set to be tested consists of 606 observations (i.e., 303 decisions by the challenger whether to use force and 303 decisions by the defender whether to use force). The crises and conflict dyads included in the analysis are listed in Appendix A; a list of crises from the revised ICB data set that were deleted or merged can be found in Appendix B.

We turn now to a discussion of the specific measurements used to operationalize our hypotheses.

## Initiation of Force

The first dependent variable measures the number of times a country was the first to use force at either minor or major levels of force during the crisis. The variable ranges from 0 to 2. If a state is the first to use a minor level of force (commit up to 1,000 troops to a combat zone) and the first to escalate the crisis to a major level of force (commit more than 1,000 troops to a combat zone), then it is coded 2. Alternatively, if one state is the first to escalate a crisis to a low level of force while the other state is the first to escalate to a high level of force, both states are coded 1. For example, in the 1969 Ussuri River confrontation between China and the former Soviet Union, both countries are coded 1. China was the first to escalate to a low level of force when it initiated a border clash at the beginning of March, but the Soviet Union was the first to escalate to a high level of force when it launched a large-scale counterattack in mid-March (Robinson 1981). In both instances, leaders made a conscious decision to resort to military force to resolve the political dispute. Finally, if neither side uses military force, the variable is coded 0 for both actors.[13]

Our measurement of the use of force includes the actions of state actors supporting nonstate actors, such as independence movements. For example, Algerian support of the Polisario independence movement in the Spanish Sahara makes Algeria a crisis participant which used force against Morocco. The expanded definition addresses, in part, Cohen's (1995) concern that the failure to include the use of surrogate forces in previous empirical research has led to a systematic underestimation of the use of force by democracies.

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[12] For each crisis, we have paired the primary challenger(s) and defender(s); the original ICB data set simply lists the actors involved in the conflict. The challenger is seeking to overturn the status quo. Although the challenger typically initiates the use of force, this need not be the case. In the 1967 Six Day War, we code Egypt as the challenger and Israel as the initiator of armed conflict.

[13] Since our data set is comprised of two decisions about the initiation of force, this creates the possibility that spatial autocorrelation will contaminate our results. Unfortunately, this problem cannot currently be accounted for in a probit model because the error terms do not conform to the usual assumptions of the ordinary-least-squares regression model. It is important to remember, however, that autocorrelation only affects the estimated standard errors of the coefficients (and thus their statistical significance), but it does not cause the coefficients to be biased (see Hanushek and Jackson 1977). In order to assess the potential for autocorrelation to exaggerate the statistical significance of our results, consider the worst case possibility: a perfect correlation between the error terms of the two states in a crisis. Under these circumstances, the proper statistical significance of our coefficients could be determined by multiplying their standard errors by the square-root of 2. The statistical results presented below remain significant even in this worst-case scenario. Moreover, the results are robust using the alternative dependent variable discussed below, which eliminates the potential dependency entirely.

### Highest Level of Force

In order to test for the robustness of our results, we included a second measure for our dependent variable. This alternative coding measures the most severe level of force used by a state in the crisis. The variable, which varies from 0 to 2, was coded for both the challenger and the defender. If the actor used more than 1,000 troops at any point during the crisis, the variable was coded 2 (or as a high level of force). If the actor used up to 1,000 troops, the variable was coded 1 (or as a low level of force). If military forces were not used, the variable was coded 0 (or no use of force). For example, in the Mayaguez crisis in 1975, the United States was coded 1 because the rescue operation involved approximately 300 troops (Head, Short, and McFarlane 1978, 141).

We wish to make this a measure of the *disproportionate* use of force in crisis behavior, a concept similar to the initiation of the use of force. To accomplish this, we also measure the highest level of force initiated by the *opposing* side as a control. Our hypotheses about the disproportionate use of force mirror those concerning *Initiation of Force.*

### Opponent's Initiation of Force

This variable is coded as the highest level of force *initiated* by the opposing state. Thus, the variable does not include uses of force by the opposing state in response to similar military actions of the state in question. If the opposing state initiated a major use of force, then this variable is coded 2. If the opponent initiated a minor use of force but did not initiate a major use, then this variable is coded 1. Finally, if the opposing state is not the first to use force at any level, then this variable is coded 0. For example, in the Six Day War in 1967, both Israel and Egypt used high levels of force, but Israel is coded 0 because its opponent did not initiate a minor or major use of force.

### Actor's Democracy

We used the Polity II data set to code for the level of democracy within each state (Gurr, Jaggers, and Moore 1989). We constructed our index by subtracting the Polity II autocracy index from the democracy index to produce a variable which ranges from −10 to +10. In order to ease the interpretation of the statistical results, this variable was rescaled from 0 to 20. The Polity II democracy index is comprised of four components: openness of executive recruitment, competitiveness of executive recruitment, competitiveness of participation, and legislative constraints on the executive. The autocracy index contains all the elements in the democracy index plus a fifth component, the regulation of participation.

We revised the Polity II codings in three ways. First, some countries were originally coded 0 on the democracy and autocracy indexes even when there was missing data for *all* the components of the indexes (e.g., Italy 1922–27). Second, the democracy and autocracy indexes are often created even when one or more of the subcomponents are missing (e.g., Cuba 1959–86). This implies, for example, that for some countries the autocracy index varies from 0 to 10, while for other countries it only varies from 0 to 7. In order to correct for these first two problems, we used the Polity II coding rules to fill in the missing data at the subcomponent level. We then reconstructed the aggregate indexes using formulas identified in Gurr, Jaggers, and Moore (1989, 37–9).[14]

Finally, we examined the subcomponent level data to ensure that codings corresponded to conventional historical accounts. The only cases in which we found the codings to be very questionable involved Japan during the interwar period. According to the Polity II data set, the Japanese legislature had authority equal to or greater than the executive from 1885 to 1945. This places the Japanese legislature in the most constraining category, making it equivalent to the British House of Commons or the U.S. Congress. We believe that the historical record indicates that only slight to moderate limitations on executive authority existed after the assassination of Prime Minister Inukai Tsuyoshi and the demise of the party system in 1932 (Beasley 1990, 181; Berger 1988).[15] We believe all three types of revisions fit within both the letter and the spirit of the coding rules described in Gurr, Jaggers, and Moore (1989).

### Opponent's Democracy

This variable was coded on the basis of the same data as the *Actor's Democracy* score and therefore ranges from 0 to 20 as well.

### Interactive-Dyadic Democracy

In order to isolate the effect of level of democracy when facing a democratic opponent, we introduced an interactive term composed of the *Actor's Democracy* score multiplied by a dummy version of the *Opponent's Democracy* score. If the latter is 17 or greater on the democracy scale, the dummy variable is coded 1. Otherwise, the variable is coded 0. Thus, when the opposing state is not democratic, this variable takes on a value of 0. When the opposing state is a democracy, however, this variable is equal to the *Actor's Democracy* score. Including this interaction as a separate variable in the analysis will allow us to identify the additional effect that the *Actor's Democracy* score has on a state's behavior *because* the opposing state is democratic.

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[14] The first problem is simply an error in the data set, probably introduced through the use of a formula which masks the difference between missing data and a value of zero. The second problem is not an error. For example, in the case of Cuba since 1959, the Polity II data set does contain values for "Competitiveness of Executive Recruitment" or "Openness of Executive Recruitment" because there has been no executive transition since Castro took over. Given that we must use an identical scale across all cases, we code the missing data based on the existing structure. In the absence of any evidence indicating that elections will be used to designate the next executive, we assume that the succession will be by selection from within the ruling elite. These first two problems affected the codings of approximately 20% of the cases.

[15] There are 11 disputes involving Japan after 1932. We have recoded the 7-point legislative constraints on the executive component of the Polity II indices from "Executive Parity or Subordination" (level 7) to "Slight to Moderate Limitations on Executive Authority" (level 3).

The threshold of 17 identifies states as democratic if they share at least the following aspects of democratic government: (1) fully competitive political participation, which implies that "there are relatively stable and enduring political groups which regularly compete for political influence at the national level" (Gurr, Jaggers, and Moore 1989, 19); (2) a popularly elected chief executive; and (3) an executive branch with at least moderate constraints on its authority. The threshold of 17 is a salient empirical break in the data between one large group of states scoring 14 or 15 on the democracy scale and a second substantial group, which scored between 18 and 20. Moreover, sensitivity analysis demonstrated that varying the threshold from 17 to 19 did not change the patterns or statistical significance of the results.

In sum, three terms were used to test the democracy-initiation hypotheses: the *Actor's Democracy* score, the *Opponent's Democracy* score, and an interaction term. When the adversary is not democratic, the interaction term is coded 0, and so the *Actor's Democracy* score term measures the influence of the actor's political structure on the likelihood of initiation. When the adversary is democratic, all three variables will generally be positive, so in this case the measure of the effect of an actor's political structure on the likelihood of initiation is determined by all three terms. Finally, as the actor becomes increasingly undemocratic, the *Actor's Democracy* score and interaction terms converge to 0, meaning that the effect of political structure on the likelihood of initiation is determined by the *Opponent's Democracy* score.

### Balance of Forces

This independent variable, which tests the realist argument that the distribution of power determines decisions to use force, measures each state's military capabilities relative to its opponent. A state's military capability is the average of three elements: number of troops, military expenditures, and military expenditures per soldier. The variable is created in four steps. First, the raw data were converted to a percentage relative to the global total of the element (e.g., actor A's troops in 1919/global total number of troops in 1919). Second, when necessary the totals were discounted to reflect the distance between the actor and the location of the conflict. The method used to discount power projection capability was developed by Bueno de Mesquita (1981, 103).[16] Third, for each element, the actor's capabilities were calculated as a percentage of the combined capabilities of both actors (e.g., actor A's troops/(actor A's troops + actor B's troops)). Finally, we averaged the three elements (troops, expenditures, expenditures per soldier). The final variable ranges from 0 to 1. A value more than 0.50 indicates that the state's military capability is superior to its opponent, and when the value approaches 1 a state then enjoys an overwhelming military advantage. The source of the troop and expenditure data was the Correlates of War Project data set, National Capabilities of States, 1816–1990.[17]

### Shared Alliance Tie

This dummy variable is included to control for the realist argument that interests, indicated by a shared alliance, are an important determinate of state behavior in crises. The variable takes on a value of 1 when the two states in the crisis share a defense pact, neutrality pact, or an entente. Otherwise, the variable is coded 0. Sources for the alliance variable included, among others, Degenhardt (1986), Oren (1990), and Small and Singer (1990).

### Satisfaction with the Status Quo

This control variable is coded 1 if a state is satisfied with the status quo with regard to the issue at stake in the crisis at the time that the crisis begins. It is coded 0 otherwise. We determined whether states were satisfied with the status quo by examining policy statements about the issue at stake immediately prior to initiation of the crisis. States which accepted the status quo as a settlement of the issue in the dispute were coded as satisfied. States which either viewed the current situation as unacceptable or were actively seeking an immediate change were coded as not satisfied. The vast majority of crises in the data set fall into two categories: territorial disputes (50%) and antiregime disputes (30%). In territorial crises, states were coded as satisfied with the status quo if they favored maintaining the current borders. In antiregime crises, states not actively seeking to overthrow another regime were coded as satisfied. States in which top foreign policy decision makers called for a revision of the territorial boundaries or the removal of opposing regimes were coded as dissatisfied. Other types of crises, such as conflicts over adherence to peace treaties and the treatment of nationals abroad, were coded in an analogous manner. In general, of course, challenging states were coded 0, while defenders were coded 1. There were a number of crises, however, such as the Indo-Chinese disputes over the North-East Frontier Agency, in which neither state was satisfied.

## DATA ANALYSIS

The first set of results to be discussed are those for the *Initiation of Force* dependent variable. For this equation, the distribution of values on the trichotomous dependent variable was as follows: never escalates (0) in 56%

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[16] Adjusted Capabilities = Composite Capabilities$^{\log(\text{miles/miles per day})+(10-e)}$.

[17] We would like to thank Professor J. David Singer for permitting us to work with the most updated version of this data set prior to its being deposited at the ICPSR.

**TABLE 1. Probit Analysis Using *Initiation of Force* Dependent Variable**

| Independent Variable | Coefficient | Standard Error | T-ratio |
|---|---|---|---|
| Actor's Democracy | −0.013 | 0.009 | 1.470 |
| Actor's Democracy × Dummy Opponent's Democracy | −0.057 | 0.018*** | 3.146 |
| Opponent's Democracy | 0.027 | 0.009** | 2.893 |
| Balance of Forces | 0.996 | 0.192*** | 5.187 |
| Shared Alliance Ties | −0.072 | 0.142 | 0.505 |
| Satisfaction with Status Quo | −1.892 | 0.183*** | 10.316 |
| Second Threshold | 0.912 | 0.084*** | 10.912 |
| Constant | −0.283 | 0.167* | 1.681 |

Number of observations = 606
Percent correctly predicted = 59%
Log likelihood at convergence = −472.6
All significance tests are one-tailed: *p > .05, **p > .01, ***p > .001.

of the cases, escalates once (1) in 24% of the cases, and escalates twice (2) in 20% of the cases ($N = 606$). This equation was estimated with an ordered probit model, and the results are presented in Table 1. On the one hand, hypothesis 1 (the monadic argument) predicts that the coefficient on the *Actor's Democracy* score will be negative and that the interaction term will be insignificant. Hypothesis 2 (the dyadic argument), on the other hand, predicts that the coefficient on the *Actor's Democracy* score will be insignificant, and the coefficient on the interaction term will be negative. Hypothesis 3, which tests a portion of the dyadic logic, predicts that the coefficient for the *Opponent's Democracy* score should be positive.

With regard to the control variables, the predictions of the hypotheses are fairly straightforward. Hypothesis 4 predicts that the coefficient on *Satisfaction with the Status Quo* will be negative. Hypothesis 5 predicts that the coefficient on the *Balance of Forces* will be positive. And hypothesis 6 predicts that the coefficient on *Shared Alliance Ties* will be negative.

The results strongly support the dyadic hypothesis and only weakly support the monadic hypothesis. The coefficient on the *Actor's Democracy* variable is slightly negative and falls just short of conventional thresholds of statistical significance. The coefficient on the interaction term, however, is sharply negative and statistically significant at the 0.001 level. These results indicate that democratic states are clearly less likely to initiate force against other democracies but only marginally less likely to initiate force against nondemocratic states. The results also provide support for hypothesis 3; the coefficient on the variable is solidly positive and statistically significant. This result implies that states are more likely to initiate force against democracies than against nondemocracies.[18]

In order to evaluate the substantive significance of these effects, we must consult the marginal impact analysis in Table 2. The table displays the probability of initiation for each level of the dependent variable and the change in probability (shown in boldface type) associated with a change in an independent variable while holding all other variables at their means or modes. The top portion of Table 2 illustrates the relative weakness of the monadic hypothesis. When the opposing state is not democratic, changing a state's domestic structure from absolutely authoritarian (0) to fully democratic (20) only decreases the probability that it will initiate twice by 8 percentage points. As the table shows, the predicted probability of initiating force twice only decreases from 32% to 24% as the *Actor's Democracy* score shifts from 0 to 20. This same change in government structure would cause a decrease of just one percentage point in the probability of initiating force once, and an increase of only 9 percentage points in the probability of never initiating force.

When the opposing state is a democracy, however, domestic structure has a powerful effect on a state's propensity to initiate the use of force. The second portion of Table 2 demonstrates that under these conditions changing a state's domestic structure from totally authoritarian (0) to a mixed system (10) decreases the probability that a state will initiate force twice by 24 percentage points. A further increase from a mixed system (10) to full democracy (20) decreases this probability by another 14 percentage points. This change in political structure from absolute autocracy to full democracy decreases the predicted probability that a state will initiate force twice against another democracy from 44% to 6% when holding other variables at their means or modes. At the same time, the change from autocracy to democracy also generates a reduction of 14 percentage points in the probability that a state will initiate force once and a very large increase of 52 percentage points in the probability that a state will never use force at all when facing a democracy.

The third portion of Table 2 examines the behavior of nondemocratic states when facing a democratic opponent. As predicted by the Bueno de Mesquita and Lalman (1992) argument presented as hypothesis 3, we find that completely autocratic states are more likely to initiate against democratic opponents. Changing the *Opponent's Democracy* score from completely authoritarian (0) to fully democratic (20) increases the predicted probability of initiating twice from 24% to 44%, or a change of 20 percentage points. In contrast, the probability of never initiating force falls by 19 percentage points. While the results cannot tell us if democratic decision makers are aware of the tendency and respond by altering their strategies, the results do support one key element of the dyadic argument: Autocracies appear to view democracies as exploitable.

The precise nature of the interactive relationship between regime type and a state's propensity to initiate force is illustrated in Figure 1. The dashed line represents the effect of a state's democracy when its opponent is not democratic, while the solid line represents the effect of democracy when facing a democratic state. The exclusively monadic argument would predict that these

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[18] The marginal analysis indicates that this result is the product of nondemocratic states initiating against democracies.

**TABLE 2. Marginal Effects on *Initiation of Force***

| | Probability (initiate twice) | Percentage Point Change from Value Above | Probability (initiate once) | Percentage Point Change from Value Above | Probability (never initiate) | Percentage Point Change from Value Above |
|---|---|---|---|---|---|---|
| Actor's Democracy with Nondemocratic Opponent | | | | | | |
| 0 | 32 | | 35 | | 33 | |
| 10 | 28 | **−4** | 35 | **0** | 37 | **4** |
| 20 | 24 | **−4** | 34 | **−1** | 42 | **5** |
| Total Change | | **−8** | | **−1** | 9 | |
| Actor's Democracy with Democratic Opponent | | | | | | |
| 0 | 44 | | 34 | | 22 | |
| 10 | 20 | **−24** | 33 | **−1** | 48 | **26** |
| 20 | 6 | **−14** | 20 | **−13** | 74 | **26** |
| Total Change | | **−38** | | **−14** | | **52** |
| Opponent's Democracy for Fully Autocratic State | | | | | | |
| 0 | 24 | | 34 | | 41 | |
| 10 | 34 | **10** | 35 | **1** | 31 | **−10** |
| 20 | 44 | **10** | 34 | **−1** | 22 | **−9** |
| Total Change | | **20** | | **0** | | **−19** |
| Balance of Forces | | | | | | |
| 1:9 | 17 | | 31 | | 52 | |
| 1:3 | 21 | **4** | 33 | **2** | 46 | **−6** |
| 1:1 | 28 | **7** | 35 | **2** | 37 | **−9** |
| 3:1 | 37 | **9** | 35 | **0** | 28 | **−9** |
| 9:1 | 43 | **6** | 34 | **−1** | 23 | **−5** |
| Total Change | | **26** | | **3** | | **−29** |
| Shared Alliance Ties | | | | | | |
| No | 28 | | 35 | | 37 | |
| Yes | 26 | **−2** | 35 | **0** | 39 | **2** |
| Satisfaction with Status Quo | | | | | | |
| No | 28 | | 35 | | 37 | |
| Yes | 1 | **−27** | 5 | **−30** | 94 | **57** |

*Note*: Predicted probabilities and percentage point change may not sum to 100% due to rounding. Marginal effects were calculated by generating predicted values from the probit model while changing the values of selected independent variables and holding the others at their means or modes. The predicted values were transformed into probabilities that the outcome would fall into each category by summing the area underneath the cumulative normal distribution between the predicted value and each of the category thresholds.

two lines should be identical. A mixed monadic-dyadic argument could account for different effects of democracy when facing differing opponents, but both of the lines should have a negative slope. The relatively flat slope of the dashed line, however, clearly demonstrates that a state's level of democracy has a limited effect on its initiation of conflict when facing a nondemocratic state. The downward sloping solid line shows that democracy does matter when opposing a democracy. Moreover, the fact that the solid line is higher than the dashed line at the left-hand side of Figure 1 is consistent with Bueno de Mesquita and Lalman's claim (1992) that authoritarian leaders view democracies as weak, which leads them to initiate the use of force more often. Notice that the solid line remains above the dashed line until the *Actor's Democracy* score reaches 5. Thus, states with an *Actor's Democracy* score of less than 5 are more likely to initiate force against a democratic opponent. Any state with an *Actor's Democracy* score greater than 5 is less likely to initiate force against a democratic state. Finally, the fact that the solid line reaches much lower than the dashed line captures one of the empirical findings that has driven research on the democratic peace. That is, when two states in a crisis are democratic, they are very unlikely to initiate force against each other.

In addition to levels of democracy, a number of other variables in our analysis also have a significant effect on *Initiation of Force*. First, as shown in Table 1, the coefficient for the *Balance of Forces* used to test the realist argument is strongly positive and statistically significant at the 0.001 level. The more powerful the state relative to its opponent, the more likely it is to initiate the use of force. In Table 2, the marginal analysis indicates that increasing the balance of power from a very unfavorable 1:9 ratio to a very favorable 9:1 ratio increases the probability of initiating force twice by 26 percentage points. While power considerations clearly play an important role, the important finding is the fact that the marginal effect of the domestic political institution variables are just as large as the realist variable. The results imply that realists who deny the importance of domestic political institutions are as incorrect as those who claim domestic factors dominate foreign policy decisions.[19] The second realist argument, which states

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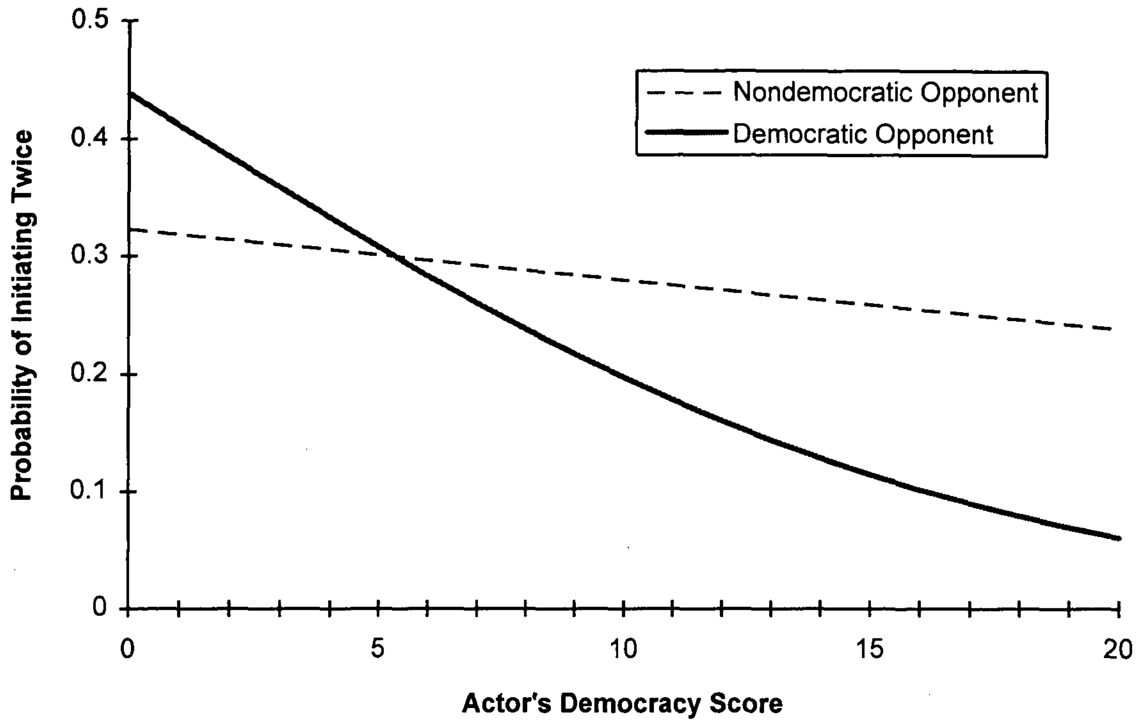
[19] A log-likelihood ratio test demonstrates that the full model is

**FIGURE 1. The Effect of *Actor's Democracy* on *Initiation of Force***



that shared alliances indicate shared interests and therefore should decrease the likelihood of conflict, is not supported by the data. The *Shared Alliance Ties* variable is neither statistically nor substantively significant. Table 1 reveals that its t-ratio is only 0.505, and Table 2 demonstrates that an alliance has virtually no effect on *Initiation of Force* in international crises. This result may seem counterintuitive, but it is consistent with some other empirical findings (Bueno de Mesquita 1981). We shall discuss some potential reasons for this findings when we turn to the issue of selection effects below.[20] Finally, being satisfied with the status quo has a very large influence on a state's propensity to initiate the use of force. Table 2 shows that changing a state from dissatisfied to satisfied decreases the probability of initiating force twice by 27 percentage points and increases the probability of never initiating force by 57 percentage points.

In order to test the robustness of these findings, we now turn to our analysis of the *Highest Level of Force* used in a crisis.[21] Once again the results clearly provide much stronger support for the dyadic hypothesis compared to the monadic hypothesis. The trichotomous dependent variable was distributed as follows: no escalation (0) in 30% of the cases, low level of escalation (1) in 32% of the cases, and high level of escalation (2) in 38% of the cases ($N = 606$). We used an ordered probit model to test our hypotheses, and the results are shown in Table 3. The coefficients for the various interactive effects of democracy should be interpreted in the same manner as discussed above.

Our results concerning the highest level of escalation virtually mirror those concerning *Initiation of Force*. Thus, the effects we document are robust across differing measures of the use of force in international crises. The coefficient on the *Actor's Democracy* variable remains slightly negative but again fails to achieve statistical significance. The interaction between *Actor's Democracy*

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significantly superior to those that restrict the model by excluding either the domestic institutional variables ($v = 16.04$, $df = 3$, $p < .01$) or the realist variables ($v = 24.32$, $df = 2$, $p < .001$). The formula used to determine significance levels is: $v = -2$[log-likelihood function value $x_1$ − log-likelihood function value $x_2$] with $x_2 - x_1$ degrees of freedom and where $x_1$ represents the restricted model and $x_2$ represents the full model. The significance level is calculated using a chi-square distribution (see Greene 1990, 356). Calculations based on the second dependent variable are even stronger ($v = 22.82$, $df = 3$, $p < .001$) and ($v = 21.12$, $df = 2$, $p < .001$) for the institutional and realist variables, respectively.

[20] Auxiliary regressions performed for all equations presented in this paper revealed that none of the variables which failed to achieve statistical significance had auxiliary $R^2$ of more than 0.46. Thus, multicolinearity cannot explain the insignificant results in any of our analyses. There is no evidence to support the Farber and Gowa (1995) claim that shared alliance ties among democracies are the real cause of the democratic peace. Moreover, in contrast to the predictions of Farber and Gowa, the coefficients are stable across both the interwar and postwar periods.

[21] In addition to using multiple dependent variables, we also conducted sensitivity analysis on various coding methods for the measures of monadic and dyadic hypotheses about the democratic peace. The statistical and substantive results remained stable when using a dummy *Opponent's Democracy* variable, incorporating dummy variables for all three institutional variables, altering the threshold for the interactive term, and incorporating a power concentration index from Polity II into the democracy variables (see Maoz and Russett 1993). The only exception was lowering the value of the threshold used in the interactive term to 16 when using the *Initiation of Force* dependent variable; the results remained robust using the *Highest Level of Force* dependent variable.

**TABLE 3. Probit Analysis Using *Highest Level of Force* Dependent Variable**

| Independent Variable | Coefficient | Standard Error | T-ratio |
|---|---|---|---|
| Actor's Democracy | −0.010 | 0.007 | 1.298 |
| Actor's Democracy × Dummy Opponent's Democracy | −0.070 | 0.018*** | 3.819 |
| Opponent's Democracy | 0.025 | 0.009** | 2.856 |
| Balance of Forces | 0.785 | 0.177*** | 4.426 |
| Shared Alliance Ties | −0.240 | 0.130* | 1.840 |
| Satisfaction with Status Quo | −1.196 | 0.129*** | 9.294 |
| Opponent's Initiation of Force | 0.867 | 0.085*** | 10.177 |
| Second Threshold | 1.031 | 0.093*** | 11.084 |
| Constant | 0.080 | 0.162 | 0.496 |

Number of observations = 606
Percent correctly predicted = 51%
Log likelihood at convergence = −561.4
All significance tests are one-tailed; *p > .05, **p > .01, ***p > .001.

and the dummy variable marking democratic opponents, however, is strongly negative and statistically significant at the 0.001 level. Finally, the coefficient on the opponent's democratic status remains strongly positive and statistically significant.

The substantive effects of the variables in this model are documented in Table 4. In generating these marginal effects the opponent's *Initiation of Force* variable was held constant at zero. Thus, these marginal effects may be interpreted as predicting the level of force used by a state given that its opponent does not initiate the use of force. That is, these effects capture the predicted probabilities of the disproportionate use of force. The top portion of Table 4 demonstrates that when the opposing state is not democratic, changes in a state's domestic structure have a limited effect on its propensity to escalate crises. The transformation from total autocracy to full democracy reduces the likelihood of using major force by only 7 percentage points. Similarly, such a change in domestic structure has no effect at all on the minor use of force and increases the probability of not using force by only 7 percentage points.

Just as was the case with *Initiation of Force*, however, when the opposing state is a democracy these results change dramatically. Table 4 shows that in this context changing a state's *Actor's Democracy* score from 0 to 20 decreases the probability of using major force by 43 percentage points. Specifically, the predicted probability that a state will use major levels of force if its opponent does not initiate falls from 48% to 5%. Moreover, the third portion of Table 4 shows that fully authoritarian states are 19 percentage points more likely to use major force against a democratic opponent than they are against an authoritarian one, even if the opposing state takes no provocative military action.

Figure 2 illustrates this interactive relationship. As with the previous analysis, the dashed line represents the effect of democracy when facing a nondemocratic state, and the solid line represents the same effect when the opponent is democratic. As one would expect, this figure matches the findings in Figure 1 in nearly every respect. The downward sloping solid line in combination with a relatively flat dashed line indicates strong support for the dyadic argument.

Once again, a number of the control variables also have a significant effect on the level of force used in a crisis. The coefficient for *Balance of Forces* is strongly positive and significant at the 0.001 level. As with *Initiation of Force*, Table 4 shows that the substantive effect of this variable is significant, but it does not overwhelm the importance of domestic structure. The most significant difference between this model and the previous equation is that *Shared Alliance Ties* have a significant effect on the disproportionate use of force. The coefficient for this variable is negative and significant at the 0.05 level. Substantively, the fact that two states in a crisis share an alliance has only a modest influence on their conflict behavior. Once again, Table 4 shows that allied states are only about 9 percentage points less likely to use major force against each other. The probability that they will make a minor disproportionate use of force does not change, while the probability that they will not use any force if their opponent does not initiate only increases by 9 percentage points. *Satisfaction with the Status Quo*, however, continues to have an extremely powerful effect on crisis behavior. The coefficient for this variable is strongly negative and significant at the 0.001 level. Table 4 illustrates that a change in status from dissatisfied to satisfied decreases the predicted probability of using major force from 34% to 5% and increases the predicted probability of never using force from 27% to 72%.

Finally, as one would expect, the opposing state's initiation of violence has a very powerful influence on a state's level of escalation in a crisis. Table 4 shows that as the opposing state shifts from not initiating force to initiating minor force, the probability that a state will use major force increases by 33 percentage points. The probability that a state will use minor force or will use no force in response, however, declines by 13 and 20 percentage points, respectively. In this context, a major use of force is still disproportionate because the opposing state has only made a small-scale use of violence. Thus, it would appear that the small-scale use of force in a crisis is not a good strategy for conveying resolve. Instead of deterring the opposition, or even eliciting a reciprocal minor use of force, this tactic appears to generate a spiraling escalation toward the major use of force. The initiation of major force by the opposition, of course, increases the probability that a state will use major force itself by 24 percentage points. Once again, the probability that a state will use minor force or will use no force declines. This time the decreases are 17 percentage points and 6 percentage points, respectively. When the opposing state initiates a major use of force, the predicted probability that a state will also use major force is 91%.

**TABLE 4. Marginal Effects on *Highest Level of Force***

| | Probability (major force) | Percentage Point Change from Value Above | Probability (minor force) | Percentage Point Change from Value Above | Probability (no force) | Percentage Point Change from Value Above |
|---|---|---|---|---|---|---|
| Actor's Democracy with Nondemocratic Opponent | | | | | | |
| 0 | 37 | | 39 | | 24 | |
| 10 | 33 | **−4** | 39 | **0** | 28 | **4** |
| 20 | 30 | **−3** | 39 | **0** | 31 | **3** |
| Total Change | | **−7** | | **0** | | **7** |
| Actor's Democracy with Democratic Opponent | | | | | | |
| 0 | 48 | | 36 | | 16 | |
| 10 | 20 | **−28** | 37 | **1** | 43 | **27** |
| 20 | 5 | **−15** | 22 | **−15** | 73 | **30** |
| Total Change | | **−43** | | **−14** | | **57** |
| Opponent's Democracy for Fully Autocratic State | | | | | | |
| 0 | 29 | | 39 | | 32 | |
| 10 | 38 | **9** | 39 | **0** | 23 | **−9** |
| 20 | 48 | **10** | 36 | **−3** | 16 | **−7** |
| Total Change | | **19** | | **−3** | | **−16** |
| Balance of Forces | | | | | | |
| 1:9 | 23 | | 39 | | 38 | |
| 1.3 | 27 | **4** | 39 | **0** | 34 | **−4** |
| 1:1 | 34 | **7** | 39 | **0** | 27 | **−7** |
| 3:1 | 41 | **7** | 38 | **−1** | 21 | **−6** |
| 9:1 | 46 | **5** | 36 | **−2** | 18 | **−3** |
| Total Change | | **23** | | **−3** | | **−20** |
| Shared Alliance Ties | | | | | | |
| No | 34 | | 39 | | 27 | |
| Yes | 25 | **−9** | 39 | **0** | 36 | **9** |
| Satisfaction with Status Quo | | | | | | |
| No | 34 | | 39 | | 27 | |
| Yes | 5 | **−29** | 23 | **−16** | 72 | **45** |
| Opponent's Initiation of Force | | | | | | |
| None | 34 | | 39 | | 27 | |
| Minor | 67 | **33** | 26 | **−13** | 7 | **−20** |
| Major | 91 | **24** | 9 | **−17** | 1 | **−6** |
| Total Change | | **57** | | **−30** | | **−26** |

*Note*: Predicted probabilities and percentage point change may not sum to 100% due to rounding. Marginal effects were calculated in the same manner as described in Table 2.

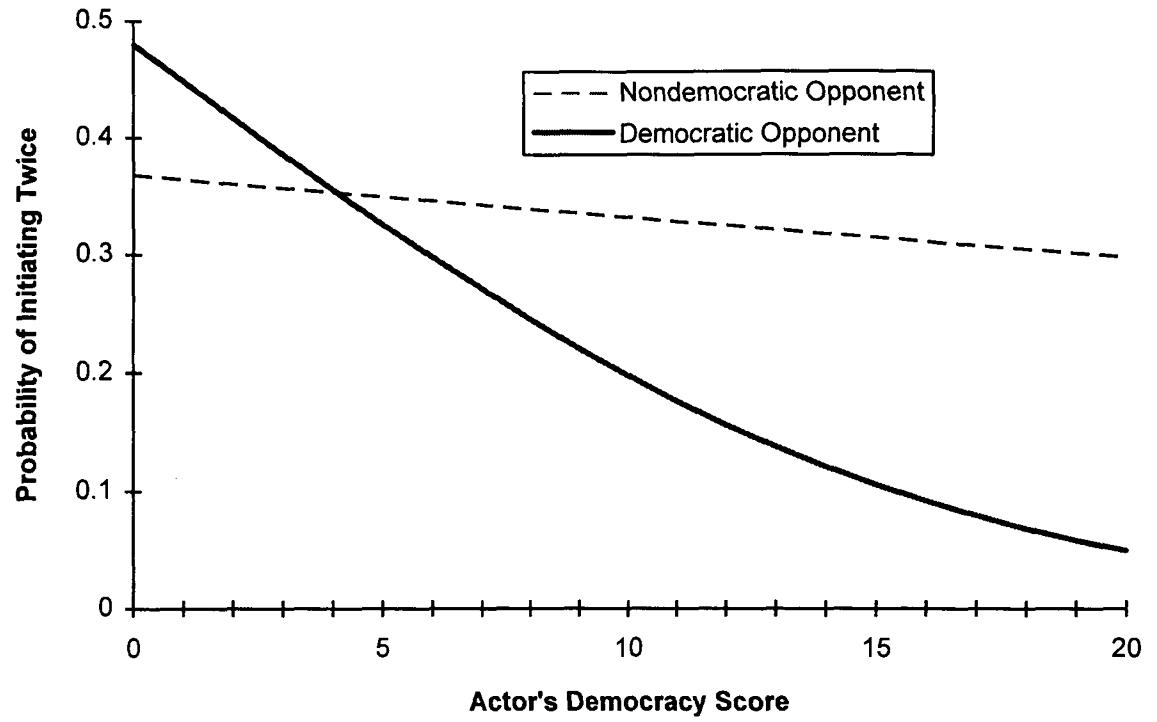
## CONSIDERATION OF SELECTION EFFECTS

Before we conclude that the monadic claims supporting the democratic peace are without merit, however, we must consider the process that leads states to become involved in international crises in the first place. Just as with the initiation of violence, a strong monadic argument can be made that democracies should be less likely to take the militarized actions that initiate international crises. If this is so, then we would expect that democratic states would systematically not select themselves into international crises as the challenger. To the extent that democracies are involved in crises, we would expect them to be disproportionately designated as defenders of the status quo. In reviewing our data set, precisely this pattern emerges. States with an *Actor's Democracy* score of 17 or greater, our threshold for qualifying as a democracy, constitute 33% of the states defending the status quo in our data set. This same group represents only 16% of the challenging states. This observation alone, however, does not demonstrate a monadic effect of democracy in selecting states into crises. It is possible, for example, that democracies' underrepresentation as challengers stems merely from their failure to challenge other democratic states.

One way to investigate the nature of this selection effect is to remove the control variable for *Satisfaction with the Status Quo* from our analysis of international crises. This variable captures the selection effect with which we are concerned, and by removing it our analysis would conflate the effect of democracy on the emergence of crises with its effect on the escalation of crises. We reestimated both probit models without the *Satisfaction with the Status Quo* variable, and the results are presented in Table 5.

In both of these equations, democracy appears to have a much stronger monadic effect. The coefficients on the *Actor's Democracy* score alone are strongly negative and statistically significant in both equations, describing an

**FIGURE 2. The Effect of *Actor's Democracy* on Disproportionate *Use of Force***



apparently monadic source for the democratic peace. Using the *Initiation of Force* dependent variable, a shift from fully autocratic to fully democratic results in a decline of 23 percentage points in the probability of using force twice. These results suggest that the failure to control for the process by which states select themselves into crises has clouded the analysis of the democratic peace by conflating the effects of *monadic selection processes* with the effects of largely *dyadic bargaining processes*. Our results concerning the selection of states into international crises must be considered preliminary, however. Future studies should use data sets that include both crisis and noncrisis behavior in order to model this process explicitly.[22]

Selection effects are not only a concern in the analysis of our democracy variables. Other variables, such as the *Balance of Forces* and *Shared Alliance Ties*, may also be influenced by selection effects. If allies are less likely to become involved in a crisis in the first place, then only the most severe alliance conflicts will appear in the crisis data set. Once a crisis in which one or both parties have threatened to use or have used military force has begun, the future value of any alliance bond may already have been discounted. This implies that the alliance variable, while strong at the dispute phase (Huth 1996), may appear very weak within crises. It is important to note, however, that the presence of selection effects does not imply that our results are biased. The coefficients we estimate are accurate assessments of the influence of the variables we consider *in international crises*. The error of selection bias occurs not in analyzing nonrandomly selected samples but in generalizing one's results to nonselected sections of the population. We have tried to avoid this error by discussing the selection process underlying our data and the effects of democracy both within crises and before crises occur.

## CONCLUSIONS AND DIRECTIONS FOR FUTURE RESEARCH

We began by noting that the conventional wisdom on the democratic peace is based on two central empirical generalizations: (1) democratic states very rarely use military force against one another, and (2) democratic states are just as likely as other states to use force against nondemocracies. While this first observation has been well documented, the evidence in support of the latter claim is actually quite thin. Some recent empirical studies have produced evidence that a democracy is less likely to use force regardless of the nature of its opponent, but they have all suffered from various problems of research design.

In general, our findings give powerful support to the argument that the democratic peace is primarily a dyadic process when addressing the escalation of international crises. Our use of the nation-state as the unit of analysis rather than the summed dyad and our focus on the initiation of violence and the disproportionate use of force allow us to distinguish more carefully between these causal processes, and in doing so our findings only weakly support those of Benoit (1994), Bremer (1992), Hewitt and Wilkenfeld (1995), and Morgan and Schwebach (1992), who all conclude that democracies are less

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[22] See Achen (1986) for a discussion of the nature of selection effects and the use of structural equations to account for the nature of this process.

**TABLE 5. Probit Analysis after Removing *Satisfaction with the Status Quo* Variable**

| | First Dependent Variable: *Initiation of Force* | | | Second Dependent Variable: *Highest Level of Force* | | |
|---|---|---|---|---|---|---|
| Independent Variable | Coefficient | Standard Errors | T-ratio | Coefficient | Standard Errors | T-ratio |
| Actor's Democracy | −0.029 | 0.008*** | 3.742 | −0.020 | 0.007** | 2.659 |
| Actor's Democracy × Dummy Opponent's Democracy | −0.045 | 0.018** | 2.483 | −0.058 | 0.016*** | 3.571 |
| Opponent's Democracy | 0.042 | 0.009*** | 4.649 | 0.036 | 0.009*** | 4.007 |
| Balance of Forces | 1.342 | 0.180*** | 7.459 | 1.036 | 0.179*** | 5.782 |
| Shared Alliance Ties | −0.042 | 0.137 | 0.306 | −0.237 | 0.128* | 1.845 |
| Opponent's Initiation of Force | — | — | — | 0.585 | 0.069*** | 8.428 |
| Second Threshold | 0.774 | 0.068*** | 11.451 | 0.937 | 0.080*** | 11.766 |
| Constant | −0.880* | 0.144*** | 6.103 | −0.311 | 0.157* | 1.982 |

Number of observations = 606
Percent correctly predicted = 60%
Log likelihood at convergence = −552.3

Number of observations = 606
Percent correctly predicted = 46%
Log likelihood at convergence = −607.2

All significance tests are one-tailed; *p > .05, **p > .01, ***p > .001

likely to escalate conflict to war in general. Once a democracy is involved in an international crisis, it carefully distinguishes the type of state with which it is bargaining and adjusts its bargaining behavior accordingly. When faced with a democratic opponent, a democracy believes that its opponent shares its desire to avoid the use of force. Without reassurances, however, democracies will be less restrained concerning the use of force.

While our analysis points to a strong dyadic effect within international crises, our examination of the *Satisfaction with the Status Quo* variable indicates that there may be substantial monadic effects for the initiation of crises. Although this finding requires more detailed analysis, our results suggest that democracies are unlikely to initiate crises with all other types of states, but once in a crisis, democracies are clearly less likely to initiate violence only against other democracies. Future work should be directed at modeling and empirically analyzing the selection process by which a much larger population of potential international disputes is reduced to a smaller number of disputes which escalate to crises and then wars. Based on our findings, such studies should document the monadic nature of this selection process, complementing the predominately dyadic effect of democracy within crises.

In addition to more sophisticated empirical analyses, better theory is needed. The dyadic argument assumes that democratic decision makers, fearing possible exploitation by their autocratic opponents, abandon their traditional norms and/or circumvent constraining institutions. The logic implies, among other things, that democracies should be prone to preemption when facing a hostile autocracy. A brief review of the 46 cases in which democracies initiated force, however, reveals that only one can be explained as an instance of democratic preemption (the Israeli attack on Egypt during the Six Day War). The assertion that democracies use force as negotiations break down seems to account for only about ten more cases, leaving a large number of initiations unexplained.[23] Moreover, the narrowness of these arguments casts them as scenarios for the use of force rather than fully developed theories. Significantly, a common theme among the examples of democratic initiation of force was that such uses seemed to be relatively popular domestically. Many of the cases, such as Israeli raids on PLO bases, involved enduring disputes in which a history of violence convinced the democratic public that force was necessary.

In consideration of these new empirical results, we offer here a preliminary theoretical framework that avoids some of the shortcomings of previous models. The new framework consists of three assumptions.

Assumption A. *Democracies conduct their foreign policies according to nonviolent norms of conflict resolution.*

Assumption B. *In a crisis, states are less likely to believe that their goals can be achieved by relying only on diplomacy and negotiation and more likely to believe that credible threats of force are important in supporting their bargaining position. In deciding whether continued negotiation is warranted, democracies assess the nature of their adversary and history of relations. If their opponent is a democracy, they infer that it shares their norms of nonviolent compromise and so believe that a negotiated settlement is still possible.*[24] *If their opponent is nondemocratic, they infer that it is not likely to share these norms. They lose confidence that negotiations and diplomatic means by themselves can be relied upon to deal with a nondemocratic adversary, particularly one*

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[23] A third dyadic argument, which has not been addressed in this paper, focuses on a crusading impulse found within democratic polities; democracies initiate conflict with nondemocracies in an effort to transform the latter into democratic states (Owen 1994; Schweller 1992). Given that at most ten cases fall within this realm, the majority of democratic initiations remain unaccounted for by existing dyadic explanations.

[24] Dixon 1994 found this expectation accurate, as democracy-democracy disputes are more likely than other disputes to end in peaceful settlement.

*which has used violence previously or has refused to compromise in prior negotiations.*

Assumption C. *When initiating force becomes an option, democratic leaders will evaluate the domestic political costs and benefits of such a strategy. When using force promises domestic political benefits, democratic leaderships will be especially likely to initiate force.*

We believe that this framework has at least three important characteristics that other approaches lack. First, it integrates structural and normative explanations of the democratic peace into a single framework, recognizing that these two explanations are complementary rather than competitive. This framework postulates that each explanation provides a necessary condition for the democratic initiation of force; a democratic government must abandon its norms of nonviolent behavior *and* feel unconstrained by domestic political pressures to initiate the use of force. Second, it accounts for the rather surprising empirical pattern of monadic behavior in the precrisis phase and dyadic behavior in the crisis phase. Most structural arguments, for example, explain why democracies initiate force when it is domestically popular, but they cannot account for why democracies initiate force almost exclusively against nondemocracies. Third, unlike some structural models that predict a dyadic pattern in crises (such as Bueno de Mesquita and Lalman 1992), our framework does not argue that democratic initiation takes the form of preemption, a scenario which very rarely occurs. But while we believe that this synthetic approach represents an improvement on previous efforts to understand the democratic peace, the true evaluation of its merits must await future research.

## APPENDIX A: 303 CONFLICT DYADS IN 272 INTERNATIONAL CRISES

(Challenger v. Defender)

| | | | |
|---|---|---|---|
| 1918 | United Kingdom v. Soviet Union<br>*Russian Civil War I* | Nicaragua v. Costa Rica<br>*Costa Rica Coup* | Japan v. Soviet Union<br>*Russian Civil War II* |
| 1919 | Poland v. Czechoslovakia<br>*Teshen* | Czechoslovakia v. Hungary<br>*Hungarian War* | Soviet Union v. Finland<br>*Russo-Finnish War* |
| | Afghanistan v. United Kingdom<br>*Third Afghan War* | Soviet Union v. Romania<br>*Bessarabia* | |
| 1920 | Poland v. Soviet Union<br>*Russo-Polish War* | Soviet Union v. Iran<br>*Persian Border* | Greece v. Turkey<br>*Greece-Turkey War I* |
| | Poland v. Lithuania<br>*Vilna I* | | |
| 1921 | Panama v. Costa Rica<br>*Costa Rica-Panama Border* | France v. Germany<br>*German Reparations* | Czechoslovakia v. Hungary<br>*Karl's Return to Hungary I* |
| | Yugoslavia v. Albania<br>*Albanian Border* | Hungary v. Austria<br>*Burgenland Dispute* | Czechoslovakia v. Hungary<br>*Karl's Return to Hungary II* |
| | Greece v. Turkey<br>*Greece-Turkey War II* | | |
| 1923 | France v. Germany<br>*Ruhr I* | Italy v. Greece<br>*Corfu Incident* | |
| 1924 | Turkey v. United Kingdom<br>*Mosul Land Dispute* | | |
| 1925 | Greece v. Bulgaria<br>*Greece-Bulgaria Front* | | |
| 1926 | Mexico v. Nicaragua<br>*Nicaragua Civil War I* | Yugoslavia v. Albania<br>*Albania* | |
| 1927 | Japan v. China<br>*Shantung I* | | |
| 1928 | Japan v. China<br>*Shantung II* | Paraguay v. Bolivia<br>*Chaco I* | |
| 1929 | China v. Soviet Union<br>*Chinese E. Railway* | | |
| 1931 | Japan v. China<br>*Mukden Incident* | | |
| 1932 | Bolivia v. Paraguay<br>*Chaco II* | Ecuador v. Colombia<br>*Leticia* | |
| 1933 | Yemen v. Saudi Arabia<br>*Saudi-Yemen War* | | |
| 1934 | Germany v. Austria<br>*Austria Putsch* | | |
| 1935 | Germany v. Lithuania<br>*Kaunas Trials* | Ethiopia v. Italy<br>*Ethiopian War* | Ecuador v. Peru<br>*Maranon I* |
| 1936 | Germany v. France<br>*Remilitarization of Rhineland* | Germany v. Spain; Italy v. Spain<br>*Spanish Civil War* | Turkey v. France<br>*Alexandretta I* |
| | Turkey v. France<br>*Alexandretta II* | Turkey v. France<br>*Alexandretta III* | |
| 1937 | Soviet Union v. Japan<br>*Amur River Incident* | China v. Japan<br>*Marco Polo Bridge* | Honduras v. Nicaragua<br>*Postage Stamp Crisis* |
| | Dominican Republic v. Haiti<br>*Haiti-Dominican Republic* | | |

## APPENDIX A: 303 CONFLICT DYADS IN 272 INTERNATIONAL CRISES continued

| | | | |
|---|---|---|---|
| 1938 | Soviet Union v. Japan<br>*Changkufeng* | Germany v. Czechoslovakia<br>*Munich* | Italy v. France<br>*Italy Threatens France* |
| 1939 | Germany v. Czechoslovakia<br>*Czech Annexation* | Germany v. Lithuania<br>*Memel* | Italy v. Albania<br>*Invasion of Albania* |
| | Japan v. Mongolia<br>*Nomonhan* | Japan v. United Kingdom<br>*Tientsin* | Germany v. Poland; Soviet Union v. Poland; Germany v. France; Germany v. United Kingdom<br>*Entry World War II* |
| | Soviet Union v. Lithuania; Soviet Union v. Latvia; Soviet Union v. Estonia<br>*Soviet Occupation of Baltics* | Soviet Union v. Finland<br>*Finnish War* | Turkey v. France<br>*Alexandretta IV* |
| 1940 | Germany v. Denmark; Germany v. Norway<br>*Invasion of Scandinavia* | Germany v. Belgium; Germany v. Netherlands; Germany v. Luxembourg<br>*Fall West Europe* | Japan v. United Kingdom<br>*Closure of Burma Road* |
| | Soviet Union v. Romania; Hungary v. Romania; Bulgaria v. Romania<br>*Rumanian Territory* | Italy v. Greece; Germany v. Yugoslavia<br>*Balkan Invasions* | |
| 1941 | United Kingdom v. Iraq<br>*Middle-East Campaign* | Germany v. Soviet Union<br>*Barbarossa* | Peru v. Ecuador<br>*Maranon II* |
| | United Kingdom v. Iran; Soviet Union v. Iran<br>*Occupation of Iran* | Japan v. United States; Japan v. Netherlands; Japan v. United Kingdom; Japan v. Thailand<br>*Pearl Harbor* | |
| 1944 | Germany v. Hungary<br>*Occupation of Hungary* | Soviet Union v. Iran<br>*Iran* | Yugoslavia v. United Kingdom<br>*Trieste I* |
| | France v. Syria<br>*Syria-French Forces* | Soviet Union v. Turkey<br>*Kars Ardahan* | Soviet Union v. Japan<br>*End of World War II* |
| | Soviet Union v. Iran<br>*Azerbaijan* | | |
| 1946 | Soviet Union v. Turkey<br>*Turkish Straits* | Yugoslavia v. Greece<br>*Greek Civil War II* | |
| 1947 | Cuba v. Dominican Republic<br>*Dominican Republic-Cuba* | India v. Pakistan<br>*Junagadh* | Pakistan v. India<br>*Kashmir I* |
| 1948 | Jordan v. Israel; Syria v. Israel; Egypt v. Israel; Iraq v. Israel; Lebanon v. Israel<br>*Israel Independence* | Soviet Union v. United States<br>*Berlin Blockade* | Nicaragua v. Costa Rica<br>*Costa Rica-Nicaragua I* |
| 1949 | Afghanistan v. Pakistan<br>*Pushtunistan I* | Guatemala v. Dominican Republic<br>*Luperon* | Soviet Union v. Yugoslavia<br>*Soviet Bloc-Yugoslavia* |
| 1950 | North Korea v. South Korea<br>*Korean War I* | | |
| 1951 | Syria v. Israel<br>*Tel Mutilah* | Pakistan v. India<br>*Punjab War* | Egypt v. United Kingdom<br>*Suez Canal* |
| 1952 | Soviet Union v. Sweden<br>*Catalina Affair* | | |
| 1953 | Taiwan v. Burma<br>*Burma* | Yugoslavia v. Italy<br>*Trieste II* | Jordan v. Israel<br>*Quibya* |
| | United States v. Guatemala<br>*Guatemala* | | |
| 1954 | China v. Taiwan<br>*Taiwan Straits I* | | |
| 1955 | Nicaragua v. Costa Rica<br>*Costa Rica-Nicaragua II* | Egypt v. Israel<br>*Gaza Raid* | Afghanistan v. Pakistan<br>*Pushtunistan II* |
| 1956 | United Kingdom v. Egypt; France v. Egypt; Israel v. Egypt<br>*Suez Nationalization* | Soviet Union v. Hungary<br>*Hungarian Uprising* | Soviet Union v. Poland<br>*Poland Liberalization* |
| 1957 | Nicaragua v. Honduras<br>*Nicaragua-Honduras* | Tunisia v. France<br>*Tunisia-France I* | Turkey v. Syria<br>*Syria-Turkey Border* |
| | Morocco v. Spain<br>*Ifni* | Soviet Union v. United States<br>*Berlin Deadline* | |
| 1958 | Tunisia v. France<br>*Tunisia-France II* | Egypt v. Sudan<br>*Sudan-Egypt Border* | China v. Taiwan<br>*Taiwan Straits II* |
| | Thailand v. Cambodia<br>*Cambodia-Thailand* | Guatemala v. Mexico<br>*Mexico Fishing Rights* | |
| 1959 | Cuba v. Haiti; Cuba v. Dominican Republic; Cuba v. Nicaragua; Cuba v. Panama<br>*Cuba-Central America I* | China v. India<br>*India-China Border I* | Iran v. Iraq<br>*Shatt Al Arab I* |
| 1960 | United Araba Republic v. Israel<br>*Rottem* | Togo v. Ghana<br>*Ghana-Togo Border* | Dominican Republic v. Venezuela<br>*Assassination Attempt-Venezuela* |
| | Zaire v. Belgium<br>*Congo I-Katanga* | Cuba v. Guatemala; Cuba v. Nicaragua<br>*Cuba-Central America II* | Somalia v. Ethiopia<br>*Ethiopia-Somalia* |

## APPENDIX A: 303 CONFLICT DYADS IN 272 INTERNATIONAL CRISES continued

| | | | |
|---|---|---|---|
| 1961 | North Vietnam v. Laos<br>*Pathet Lao Offensive I* | United States v. Cuba<br>*Bay of Pigs* | Afghanistan v. Pakistan<br>*Pushtunistan III* |
| | Soviet Union v. United States<br>*Berlin Wall* | Iraq v. Kuwait<br>*Kuwait Independence* | Tunisia v. France<br>*Bizerta* |
| | Indonesia v. Netherlands<br>*West Irian II* | Egypt v. Syria<br>*Breakup of UAR* | India v. Portugal<br>*Goa II* |
| | N. Vietnam v. S. Vietnam<br>*Vietcong Attack* | | |
| 1962 | Mali v. Mauritania<br>*Mauritania-Mali* | Taiwan v. China<br>*Taiwan Straits III* | North Vietnam v. Laos<br>*Pathet Lao Offensive II* |
| | India v. China<br>*India-China Border II* | Saudi Arabia v. North Yemen<br>*Yemen War-Cluster I* | United States v. Soviet Union<br>*Cuban Missile Crisis* |
| 1963 | Indonesia v. Malaysia<br>*Malaysia Federation* | Haiti v. Dominican Republic<br>*Haiti-Dominican Republic* | Morocco v. Algeria<br>*Morocco-Algeria Border* |
| | Somalia v. Kenya<br>*Kenya-Somalia* | Greece v. Turkey<br>*Cyprus I* | Jordan v. Israel<br>*Jordan Waters* |
| | Dahomey v. Niger<br>*Niger-Dahomey* | Burundi v. Rwanda<br>*Rwanda-Burundi* | |
| 1964 | Somalia v. Ethiopia<br>*Ogaden I* | North Vietnam v. United States<br>*Gulf of Tonkin* | Saudi Arabia v. North Yemen<br>*Yemen War-Cluster III* |
| 1965 | N. Vietnam v. S. Vietnam<br>*Pleiku* | Pakistan v. India<br>*Rann of Kutch* | Pakistan v. India<br>*Kashmir II* |
| | Rhodesia v. Zambia<br>*Rhodesia's UDI* | | |
| 1966 | North Yemen v. Saudi Arabia<br>*Yemen War-Cluster IV* | Jordan v. Israel<br>*El Samu* | |
| 1967 | Cuba v. Bolivia<br>*Che Guevara/Bolivia* | Egypt v. Israel; Syria v. Israel; Jordan v. Israel<br>*Six Day War* | Greece v. Turkey<br>*Cyprus II* |
| 1968 | North Korea v. United States<br>*Pueblo* | Jordan v. Israel<br>*Karameh* | Soviet Union v. Czechoslovakia<br>*Prague Spring* |
| | Venezuela v. Guyana<br>*Essequibo Territory* | Egypt v. Israel<br>*Pre-War of Attrition* | Israel v. Lebanon<br>*Beirut Airport* |
| 1969 | China v. Soviet Union<br>*Ussuri River* | Egypt v. Israel<br>*War of Attrition I* | North Korea v. United States<br>*EC-121 Spy Plane* |
| | Iraq v. Iran<br>*Shatt Al Arab II* | El Salvador v. Honduras<br>*Football War* | Syria v. Leganon<br>*Cairo Agreement-PLO* |
| 1970 | North Vietnam v. Cambodia<br>*Invasion of Cambodia* | Syria v. Jordan<br>*Black September* | Guinea v. Portugal<br>*Portuguese Invasion of Guinea* |
| 1971 | India v. Pakistan<br>*Bangladesh* | Libya v. Chad<br>*Chad-Libya* | Zambia v. South Africa<br>*Caprivi Strip* |
| | Tanzania v. Uganda<br>*Uganda/Tanzania I* | | |
| 1972 | Tanzania v. Uganda<br>*Uganda/Tanzania II* | North Yemen v. South Yemen<br>*North-South Yemen I* | |
| 1973 | Zambia v. Rhodesia<br>*Zambia* | Iraq v. Kuwait<br>*Iraq Invasion of Kuwait* | Egypt v. Israel<br>*Israel Mobilization* |
| | Iceland v. United Kingdom<br>*Cod War I* | Egypt v. Israel; Syria v. Israel<br>*Yom Kippur War* | South Yemen v. Oman<br>*South Yemen-Oman* |
| 1974 | Greece v. Cyprus; Turkey v. Cyprus<br>*Cyprus III* | | |
| 1975 | Cambodia v. United States<br>*Mayaguez* | Zaire v. Angola<br>*War in Angola* | Morocco v. Spain<br>*Moroccan March* |
| | Guatemala v. United Kingdom<br>*Belize I* | Algeria v. Morocco; Algeria v. Mauritania<br>*Sahara* | Iceland v. United Kingdom<br>*Cod War II* |
| | Indonesia v. Portugal<br>*East Timor* | | |
| 1976 | Syria v. Lebanon<br>*Lebanon Civil War I* | Uganda v. Kenya<br>*Uganda Claims* | Mozambique v. Rhodesia<br>*Operation Thrasher* |
| | Iraq v. Syria<br>*Iraqi Threat* | Israel v. Uganda<br>*Entebbe Raid* | Libya v. Sudan<br>*Sudan Coup Attempt* |
| | Turkey v. Greece<br>*Aegean Sea* | Syria v. Israel<br>*Syria Mobilization* | Botswana v. Rhodesia<br>*Operation Tangent* |
| 1977 | Angola v. Zaire<br>*Shaba I* | Guatemala v. United Kingdom<br>*Belize II* | Egypt v. Libya<br>*Egypt-Libya Border* |
| | Somalia v. Ethiopia<br>*Ogaden II* | Zambia v. Rhodesia<br>*Rhodesia Raids* | Vietnam v. Cambodia<br>*Vietnam Invasion of Cambodia* |
| | Algeria v. France<br>*French Hostages* | Argentina v. Chile<br>*Beagle Channel I* | |

## APPENDIX A: 303 CONFLICT DYADS IN 272 INTERNATIONAL CRISES continued

| | | | |
|---|---|---|---|
| 1978 | Libya v. Chad<br>*Chad-Libya II* | Syria v. Lebanon<br>*Lebanon Civil War II* | Cambodia v. Thailand; China v. Vietnam<br>*Sino-Vietnam War* |
| | Lebanon v. Israel<br>*Litani Operation* | Angola v. South Africa<br>*Cassinga Incident* | Angola v. Zaire<br>*Shaba II* |
| | Costa Rica v. Nicaragua<br>*Nicaraguan Civil War* | Argentina v. Chile<br>*Beagle Channel II* | Uganda v. Tanzania<br>*Fall of Amin* |
| 1979 | South Yemen v. North Yemen<br>*North-South Yemen II* | Soviet Union v. Afghanistan<br>*Afghanistan Invasion* | Iran v. United States<br>*U.S. Hostages-Iran* |
| 1980 | Libya v. Tunisia<br>*Raid on Gafsa* | Soviet Union v. Poland<br>*Solidarity* | Libya v. Malta<br>*Malta-Libya Oil Dispute* |
| | Iraq v. Iran<br>*Iran-Iraq War* | Libya v. Gambia<br>*Libya Intervention in Gambia* | Syria v. Jordan<br>*Jordan-Syria Confrontation* |
| | Somalia v. Ethiopia<br>*East Africa Confrontation* | | |
| 1981 | Libya v. Chad<br>*Chad-Libya Merger* | Ecuador v. Peru<br>*Peru-Ecuador* | Mozambique v. South Africa<br>*Mozambique Raid* |
| | Venezuela v. Guyana<br>*Essequibo II* | Israel v. Syria<br>*Al-Biqua Missiles* | Cameroon v. Nigeria<br>*Nigeria-Cameroon Border* |
| | Israel v. Iraq<br>*Iraq Nuclear Reactor* | Libya v. United States<br>*Gulf of Syrte I* | Iran v. Bahrain<br>*Coup Attempt-Bahrain* |
| 1982 | Argentina v. United Kingdom<br>*Falklands-Malvinas* | Israel v. Lebanon<br>*Lebanon War* | Ethiopia v. Somalia<br>*Ogaden III* |
| | Lesotho v. South Africa<br>*Lesotho Raid* | | |
| 1983 | Libya v. Sudan<br>*Libya Threat-Sudan* | Chad v. Nigeria<br>*Chad-Nigeria Clashes* | Libya v. Chad<br>*Chad-Libya VI* |
| | United States v. Grenada<br>*Invasion of Grenada* | Botswana v. Zimbabwe<br>*Botswana-Zimbabwe Border* | Ethiopia v. Sudan<br>*Sudan-Ethiopia Border* |
| 1984 | Thailand v. Vietnam<br>*Thai Border Incident* | Libya v. Sudan<br>*Omduran Bombing* | China v. Vietnam<br>*Sino-Vietnam Clashes* |
| | Laos v. Thailand<br>*Village Border Crisis I* | United States v. Nicaragua<br>*Nicaragua Mig-21's* | |
| 1985 | Botswana v. South Africa<br>*Botswana Raid* | Libya v. Tunisia<br>*Expulsion of Tunisians* | Burkina Faso v. Mali<br>*Burkina Faso-Mali Border* |
| 1986 | Libya v. United States<br>*Gulf of Syrte II* | Qatar v. Bahrain<br>*Bahrain-Qatar Dispute* | Zimbabwe v. South Africa<br>*South Africa Cross-Border Raid* |
| | Sudan v. Uganda<br>*Rebel Attack-Uganda* | Malawi v. Mozambique<br>*Mozambique Ultimatum* | Ghana v. Togo<br>*Attempted Coup-Togo* |
| | Honduras v. Nicaragua<br>*Honduras-Nicaragua Border* | | |
| 1987 | China v. Vietnam<br>*China-Vietnam Border* | India v. Pakistan<br>*Punjab War Scare II* | Ethiopia v. Somalia<br>*Somalia-Ethiopia Border* |
| | Turkey v. Greece<br>*Aegean Sea II* | Cameroon v. Nigeria<br>*Nigeria-Cameroon Border* | India v. Sri Lanka<br>*India Intervention in Sri Lanka* |
| | Iran v. Saudi Arabia<br>*Mecca Pilgrimage* | Laos v. Thailand<br>*Village Border Crisis II* | Kenya v. Uganda<br>*Kenya-Uganda Border* |
| 1988 | China v. Vietnam<br>*Spratly Islands* | | |

## APPENDIX B: 121 DELETED OR MERGED ICB CRISES

### Intrawar Crises (27)

- *Smyrna* 1919
- *Cilician War* 1920
- *Battle of Britain* 1940
- *East African Campaign* 1940
- *El Alamein* 1942
- *Stalingrad* 1942
- *Fall of Italy* 1943
- *Soviets in Eastern Europe* 1944
- *D-Day* 1944
- *Fall of Saipan* 1944
- *Leyte and Luzan* 1944
- *Final Soviet Offensive* 1945
- *Iwo Jima* 1945
- *Okinawa* 1945
- *Sinai Incursion* 1948
- *Korean War II* 1950
- *Korean War III* 1953
- *Tet Offensive* 1968
- *Vietnam Spring Offensive* 1969
- *War of Attrition II* 1970
- *Invasion of Laos II* 1971
- *Vietnam Ports Mining* 1972
- *Christmas Bombing* 1972
- *Final Vietnam Offensive* 1974
- *Khorramshahr* 1982
- *Basra-Kharg Island* 1984
- *Iraq Recapture-Fao* 1988

### No Threat A—No Strong Evidence that the Challenger Considered Using Force (26)

- *Aaland Islands* 1919
- *Rhenish Rebellions* 1920
- *Austrian Separatists* 1921
- *Ruhr II* 1924
- *Haiti Unrest* 1929
- *Bulgaria-Turkey I* 1935
- *Bulgaria-Turkey II* 1935
- *Panay Incident* 1937
- *Czech May Crisis* 1938
- *Baghdad Pact* 1955
- *West Irian I* 1957
- *Jordan Regime* 1957
- *Aborted Coup-Indonesia* 1958
- *Formation of UAR* 1958
- *Mali Federation* 1960
- *Cuba-Venezuela* 1963
- *Guinea Regime* 1965
- *Cienfuegos Sub Base* 1970
- *Libyan Plane* 1973
- *Soviet Threat-Pakistan* 1979
- *Nicaragua-Colombia* 1979
- *Libyan Threat-Sadat* 1980
- *Coup Attempt-Gambia* 1981
- *Aegean Naval Crisis* 1984
- *Al Biqua Missiles* 1985
- *Egypt-Libya Tensions* 1985

### No Threat B—No Government-to-Government Threat (13)

- *Assassination of King Alexander* 1934
- *Greek Civil War I* 1944
- *Truman Doctrine* 1947
- *China Civil War* 1948
- *East Berlin Uprising* 1953
- *Goa I* 1955
- *Lebanon-Iraq Upheaval* 1958
- *Jordan Internal Challenge* 1963
- *Panama Canal* 1964
- *East Africa Rebellions* 1964
- *Congo II* 1964
- *Dominican Republic* 1965
- *Syria in Lebanon* 1987

### Capitulation Crises (9)

- *Anschluess* 1938
- *Polish Ultimatum* 1938
- *Communism in Rumania* 1945
- *Communism in Poland* 1946
- *Communism in Hungary* 1947
- *Marshall Plan* 1947
- *Communism in Czechoslovakia* 1948
- *Soviet Note to Finland I* 1948
- *Soviet Note to Finland II* 1961

### Merged Crises (38)

- *Transcaucasia* 1917
  - w/ *Russian Civil War I* 1918
- *Vilna II* 1920
  - w/ *Vilna I* 1920
- *Greece-Turkey War III* 1922
  - w/ *Greece-Turkey War II* 1921
- *Chanak* 1922
  - w/ *Greece-Turkey II* 1921
- *Shanghai* 1932
  - w/ *Mukden* 1931
- *Jehol Campaign* 1933
  - w/ *Mukden* 1931
- *Walwal* 1934
  - w/ *Ethiopian War* 1935
- *Danzig* 1939
  - w/ *Entry WWII* 1939
- *Palestine Partition* 1947
  - w/ *Israel Independence* 1948
- *Qalqilya* 1956
  - w/ *Quibya* 1953
- *Suez-Sinai Campaign* 1956
  - w/ *Suez Nationalization* 1956
- *Yemen War-Cluster II* 1964
  - w/ *Yemen War I* 1962
- *Nouakchott I* 1976
  - w/ *Sahara* 1975
- *Nagomia Raid* 1976
  - w/ *Operation Thrasher* 1976
- *Chimolo Tembue Raids* 1977
  - w/ *Operation Thrasher* 1976
- *Mapai Seizure* 1977
  - w/ *Operation Thrasher* 1976
- *Nouakchott II* 1977
  - w/ *Sahara* 1975
- *Chad-Libya III* 1978
  - w/ *Chad-Libya II* 1978
- *Air Rhodesia Incident* 1978
  - w/ *Rhodesia Raids* 1977
- *Angola-Invasion Scare* 1978
  - w/ *Cassinga* 1978
- *Tan Tan* 1979
  - w/ *Sahara* 1976
- *Raids on Zipra* 1979
  - w/ *Rhodesia Raids* 1977
- *Raids on Swapo* 1979
  - w/ *Cassinga* 1978
- *Chad-Libya IV* 1979
  - w/ *Chad-Libya II* 1978
- *Goulimime Tarfaya Road* 1979
  - w/ *Sahara* 1975
- *Rhodesia Settlement* 1979
  - w/ *Operation Tangent* 1976, *Operation Thrasher* 1976, and *Rhodesia Raids* 1977
- *Raid on Angola* 1979
  - w/ *Cassinga* 1978
- *Operation Iman* 1980
  - w/ *Sahara* 1975
- *Operation Smokeshell* 1980
  - w/ *Cassinga* 1978
- *Operation Protea-Angola* 1981
  - w/ *Cassinga* 1978
- *Polisario Attack* 1981
  - w/ *Sahara* 1975
- *Operation Askari* 1983
  - w/ *Cassinga* 1978
- *South Africa Raid on Lesotho* 1985
  - w/ *Lesotho Raid* 1982
- *Chad-Libya VII* 1986
  - w/ *Chad-Libya VI* 1979
- *Chad-Libya VIII* 1986
  - w/ *Chad-Libya VI* 1979
- *Western Sahara* 1987
  - w/ *Sahara* 1975
- *S. Africa Intervention Angola* 1987
  - w/ *Cassinga* 1978
- *Sandanista Border Crossing* 1988
  - w/ *Honduras-Nicaragua* 1986

### Colonial Crises (5)

- *Indonesia Independence I* 1945
- *Indonesia Independence II* 1947
- *Indonesia Independence II* 1948
- *Invasion of Laos I* 1953
- *Dien Bien Phu* 1954

### One or Both Parties Are Not Recognized Members of the International System (3)

- *Baltic Independence* 1918
- *Nejd-Hijaz War* 1924
- *Hyderabad* 1948

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# The Costs of Coalition: Coalition Theories and Italian Governments

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*Governments in Italy both change and remain the same. From 1946 to 1992 the Christian Democratic Party always held governing power. But almost no cabinet stayed in office more than a few years, and many governments collapsed after only a few months. How can instability coexist with stability in this way? How can governments break up at such low cost and with so little effect on alternation? These questions are rooted in—but not resolved by—the available game-theoretic literature on coalitional behavior. My answer is that politicians' purposive actions can reduce the costs of coalition. I argue that the costs of making, breaking, and maintaining coalitions depend on political institutions and on the array of parties and voters in policy space. Institutional and spatial conditions structure politicians' opportunities and attempts to lower costs. Under some conditions, as I show, coalitions are cheap, and politicians can easily make coalitions even cheaper. The inference is that this framework comprehends both Italy's extremes and the degrees of stability found in other parliamentary democracies.*

The record of Italian governments displays a pattern that is deeply perplexing. As Figure 1 shows, from 1946 to 1992 cabinets in Italy both changed and remained the same. The Christian Democratic Party [Democrazia Cristiana (DC)] always held governing power. But almost no government stayed in office more than a few years, and many governments collapsed after only a few months. Although Italy exhibited the lowest turnover rate of any parliamentary democracy (K. Strom 1990b, 128),[1] it had, except for the defunct French Fourth Republic, the most short-lived governments (King et al. 1990, 867). How can instability coexist with stability in this way? How can governments break up at such low cost and with so little effect on alternation?

In posing and pursuing these questions, I am guided by the game-theoretic literature on coalitional behavior. My question about (in)stability reflects a central—perhaps *the* central—result in this literature: Voting games in multidimensional policy space under simple majority rule are subject to endless cycles among alternative decisions (e.g., McKelvey 1976, 1979; Schofield 1983). Given these predictions, it is a mystery why real-world legislative decision making exhibits substantial stability. Efforts to comprehend the inconsistency between predictions of chaos and empirical observations of decisional stability have stimulated the development of entire schools of spatial theory (for reviews, see Schofield, Grofman, and Feld 1988; G. Strom 1990). Such efforts inform my study. But I move beyond extant theoretical and empirical research in order to deal with an anomaly that to date has not been adequately explored and understood: the combination of decisional stability and instability found in Italy.

My question about costs, too, is rooted in the body of work on coalitions. An implicit but widely shared assumption of theorists is that government coalitions, once installed, can withstand much internal tension because their members want to avoid the costs associated with destroying a coalition. As Figure 1 suggests, however, such costs seem to have been very limited in Italy before 1992. Why? The available literature clarifies the question but does not provide an answer to it.

The answer offered here is that politicians' purposive actions can reduce the costs of coalition. I argue that the costs of making, breaking, and maintaining coalitions depend on political institutions and on the array of parties and voters in policy space. Institutional and spatial conditions structure politicians' opportunities and attempts to lower costs. Under some conditions, as I demonstrate, coalitions are cheap, and politicians can easily make coalitions even cheaper.

Such conditions, I contend, are not unique to Italy. Figure 2 illustrates the broad correlation between cabinet duration and alternation in office.[2] Governments in Italy, the French Fourth Republic, Israel, and Portugal (and to a degree Finland and Belgium) last a short while and are subject to limited turnover. Ireland, Iceland, and Norway (and to some extent Canada and the United Kingdom) evince long duration and high alternation. In Sweden, Spain, Denmark, and the Netherlands, cabinets attain an average duration resembling that of the second group but experience fairly restricted turnover. Italy is an extreme case, then, and looks anomalous in light of coalition theories, but it is not sui generis. Scrutiny of this case gives special leverage, for Italy's extremity points up that a common, key assumption among analysts—the notion that coalitions are costly to build and break—fails to hold under certain conditions. I reason that, in Italy (and France, Israel, Portugal, Finland, and Belgium), institutional and spatial conditions curb the costs of coalition and induce politicians to try to lower costs further.

Such conditions are not impervious to change. The logic presented here implies that, if political institutions or configurations of policy space are redrawn, then changes in patterns of coalition politics should ensue.

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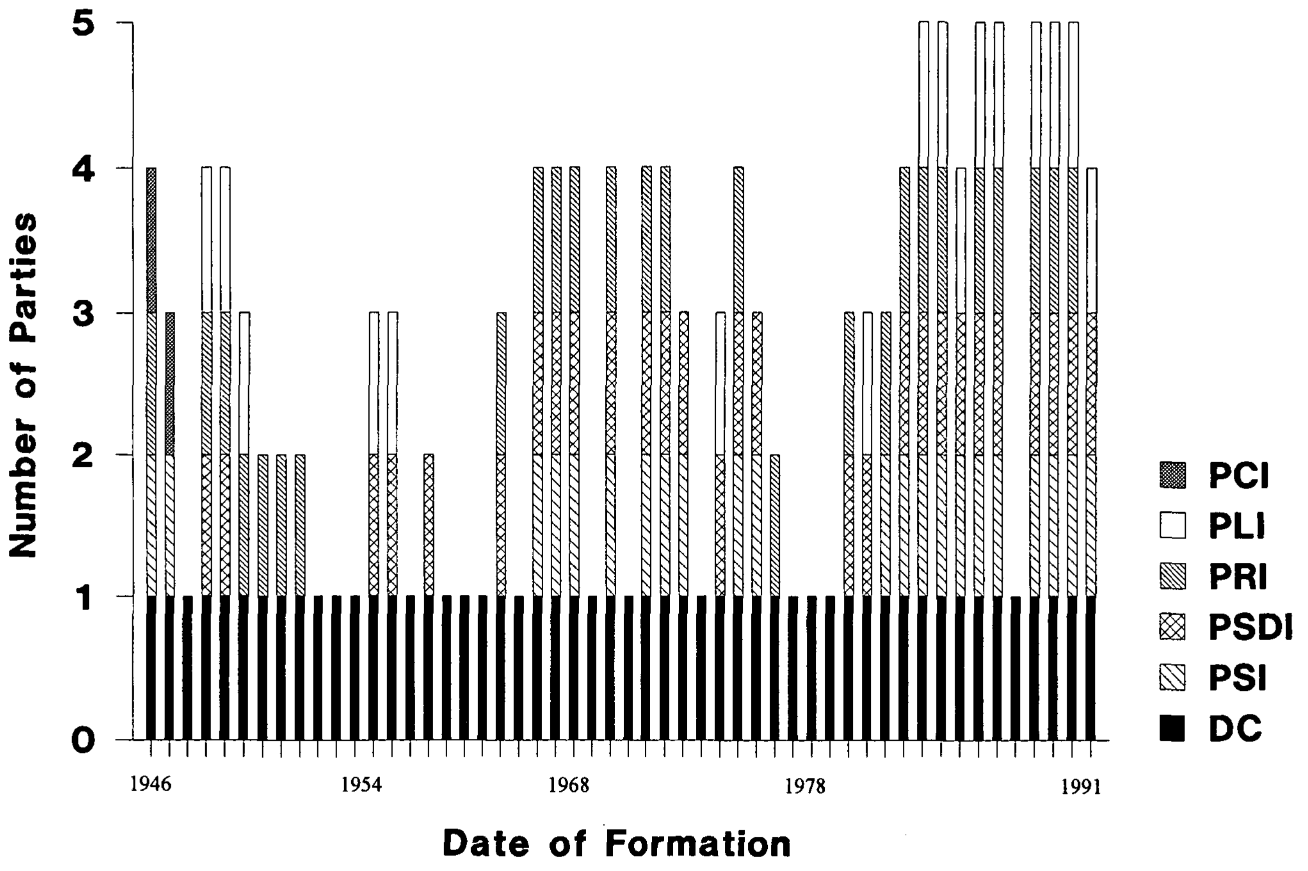
[1] Kaare Strom defines turnover or alternation as "the proportion of legislative seats held by parties changing status between government and opposition," averaged across a country's governments (1990b, 125).

[2] My thanks to Kaare Strom for providing me with these data. On the measure of alternation (or rate of turnover), see note 1. On the criteria used to define government duration, see note 9. The Pearson correlation between alternation and duration is 0.575.

**FIGURE 1. The Composition of Italian Governments, 1946–92**



*Note:* Governments are ordered chronologically on the horizontal axis, with tick marks representing separate cabinets. For simplicity, this figure always depicts the Socialists (PSI) and Social Democrats (PSDI) as separate parties, even though they were united before 1947 and between 1966 and 1969.

Intertemporal comparisons allow for tests of this implication. Most of the data I examine cover the years 1946–92. The DC dominated the Italian policy space throughout this period. From 1976 to 1992, however, electoral outcomes rendered the Socialists [Partito Socialista Italiano (PSI)] the essential component of any DC-led coalition able to control a majority while excluding the Communists [Partito Comunista Italiano (PCI)]. The costs of coalition from 1976 to 1992 thus should have differed from costs incurred earlier. Differences do appear, as detailed below. Moreover, recent events furnish clinching evidence on variations across time. The 1992 elections registered far-reaching change in both spatial and institutional conditions in Italy. Coalition politics was transformed.

The first section of the article assesses the treatment of costs in the literature on coalition bargaining. The second section distinguishes several kinds of costs attached to coalitions and advances an explanation for variations in those costs across party systems. The third part shows that my explanation accounts for features of Italian politics that are otherwise inexplicable. I conclude by discussing how my argument may be applied in cross-national analyses.

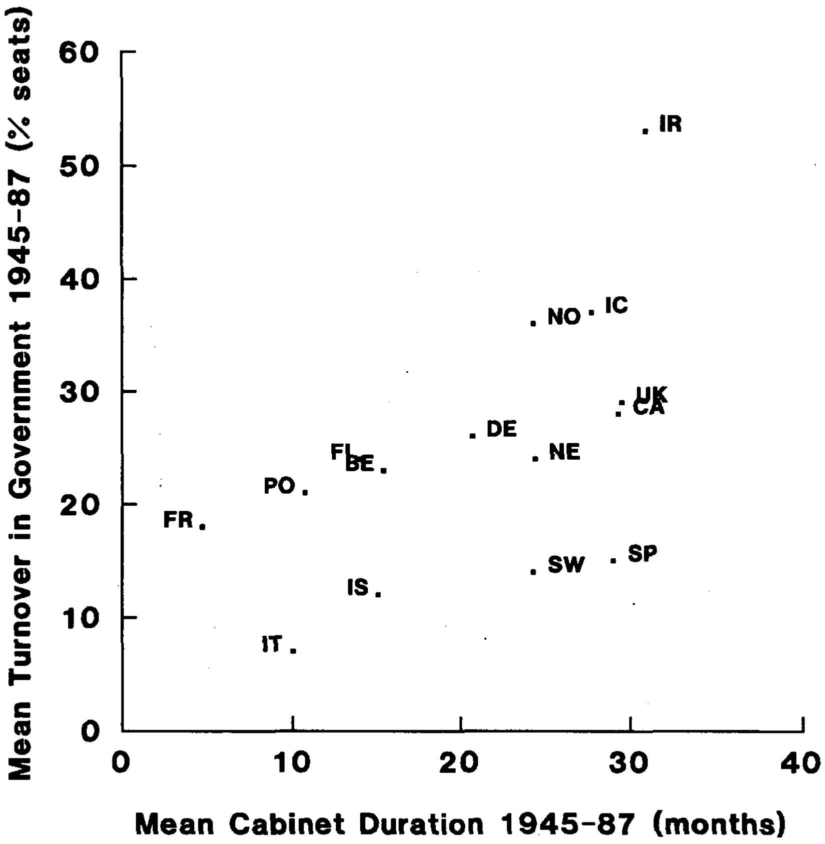
## COSTS IN COALITION THEORIES

I outline predictions from four schools of research on coalitions and identify the anomalies that emerge when each prediction meets the record of coalition governments. A repeated theme in my survey is the costs and benefits of erecting and undoing coalitions. The extensive cross-national aggregate evidence already accumulated enables me to devote intensive attention to Italy, an outlier in study after study.

### Office-Driven, Institution-Free, Cooperative Theories

Riker (1962) predicts the formation of minimal winning coalitions, which would lose their parliamentary majority if any member party were to withdraw from them. This model highlights the costs of enlarging coalitions and operationalizes costs in forgone cabinet portfolios. In closely related theories, Leiserson (1968) predicts as most likely those minimal winning coalitions that have the smallest number of parties, and Axelrod (1970) predicts minimal connected winning coalitions, which join parties adjacent to one another on a policy spectrum

FIGURE 2. Cabinet Duration and Alternation in Office in Fifteen Parliamentary Democracies



BE = Belgium; CA = Canada; DE = Denmark; FI = Finland; FR = Fourth Republic France; IC = Iceland; IR = Ireland; IS = Israel; IT = Italy; NE = Netherlands; NO = Norway; PO = Portugal; SP = Spain; SW = Sweden; and UK = United Kingdom.

and which would either slip below a majority or stop being connected if any party were to withdraw. Here, bargaining is pictured as a costly activity. Politicians save on bargaining costs by minimizing the number of partners (for Leiserson) or by spanning differences in policy across partners (for Axelrod).

Tests of these theories indicate that the minimal connected winning prediction performs most successfully, and the prediction of minimal winning coalitions ranks second (e.g., Franklin and Mackie 1984, 676). The latter is usually inaccurate in Italy. Almost all Italian governments have been either minority cabinets (less than winning) or surplus coalitions (more than minimal). Of the 52 governments from 1946 to 1992, only one unambiguously qualifies as a minimal winning coalition, and even that cabinet contradicts the logic of minimal winning theory.[3] Moreover, a recent twelve-nation study finds that the minimal connected winning prediction fares least well in Denmark, Finland, Ireland, Italy, and Norway (Laver and Schofield 1990, 100).

## Policy-Driven, Institution-Free, Cooperative Theories

Whereas office-driven predictions deal primarily with coalition size and are often fulfilled by coalitions of different parties, policy-driven theories predict policy outcomes that are often consistent with coalitions of different size. Theories based on the assumption of unidimensional policy space (De Swaan 1973) predict that the party containing the median legislator will control policy and can govern alone. Multidimensional models predict that a party will control policy and will govern if it occupies the core—a policy position that cannot be overturned, given the overall configuration of actors' sizes and positions (e.g., McKelvey and Schofield 1987; Schofield 1986, 1993). For a core party, allies represent no gain in policy (which the core party already dictates); a core party is thus likely to form a minority cabinet. This school expects lasting governments only in the presence of a stable core (Schofield, Grofman, and Feld 1988, 206).

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[3] The three-party cabinet built by Amintore Fanfani in 1962 controlled 51% of the seats in the Chamber of Deputies, whereas three other coalitions held exactly half. The PSI abstained on Fanfani's vote of investiture, extending indirect support (and refraining from direct opposition) in a way that a pure search for power would render superfluous. The architects of the Fanfani government saw the arrangement as a second-best solution, designed to pave the way for the larger, long-term alliance they preferred, an alliance embracing the PSI. Below, I discuss Italy's other minimal winning coalitions, formed after the 1992 elections.

Despite its considerable achievements, this class of theories does little to illuminate quick collapses of cabinets in Italy. More than 80% of all governments in twelve parliamentary democracies, including 100% in Italy and the Netherlands, either incorporate or are supported by the median party on the left-right spectrum (Laver and Schofield 1990, 113–9). Party systems in Italy, the Netherlands, and several other countries are best modeled as two-dimensional, however (p. 136, drawing on Budge, Robertson, and Hearl 1987). In Italy, the two-dimensional configuration of parties' sizes and positions qualifies the DC as a core party from 1946 through 1992. The prediction is thus that the DC should always govern, and this has indeed been true. But it would be inexplicable within this reasoning if two fragmented systems with strong core parties (such as Italy and the Netherlands) featured cabinets with substantially different average durations, which is in fact the case. Why do Italian governments display shorter tenures (King et al. 1990, 867) and yet also lower turnover in office (K. Strom 1990b, 128) than do Dutch governments? If the balance of costs and benefits brings core parties to form minority governments, why are minority cabinets much less common in the Netherlands than in Italy?

## Policy-Driven, Institution-Focused, Noncooperative Theories

An influential school examines how institutions restrict the alternatives open to politicians and thus shape the coalitions they build (Shepsle 1979, 1986; Shepsle and Weingast 1987). For example, Laver and Shepsle (1990a, 1990b, 1990c) posit institutions in which "the policy outcomes associated with any given government (and thereby payoffs to legislators) can be forecast from the policy preferences of those nominated to hold key cabinet portfolios" (1990c, 4). They show for the three-party, two-dimensional case that limitations on credible commitments can produce an equilibrium minority cabinet. The requirements of credibility can also yield oversized coalitions. This argument about surplus coalitions makes sense of an outcome that is not explained by minimal winning theory, that is not uncommon, and that is frequent in Finland, Italy, and the Netherlands (Laver and Schofield 1990, 71; on surplus coalitions in consociational democracies, see Lijphart 1984). Another prediction fares less well when fit against actual governments. With few exceptions, minority cabinets tend to form in systems with more than three parties, such as Denmark, Italy, and Sweden; minority cabinets rarely form in three-party, two-dimensional systems, such as Austria and Germany (K. Strom 1990b; cf. Schofield 1993, 8–9).[4]

## Theory-Oriented Empirical Research

A large body of research on coalition composition and durability incorporates ideas from the game-theoretic literature but does not use formal deductive methodology (e.g., Dodd 1976; Franklin and Mackie 1983; Lijphart 1984; Powell 1982; Taylor and Herman 1971). One prominent contributor is K. Strom (1990b), who contends that minority cabinets tend to result when policy benefits from governing are low (when strong parliamentary committees give opposition parties the opportunity to influence policy) and, above all, when electoral costs are high (when elections are competitive and decisive, so that incumbency carries a penalty, and bargaining power hinges on electoral verdicts). Strom (p. 151) expects minorities to govern with "intermediate frequency" in Italy, since electoral costs and policy benefits from governing there are low. As he documents, however, the percentage of minority cabinets in Italy stands "well above the mean" for the fifteen countries he examines (p. 132). Why? Strom cites electoral costs, even though they are relatively low (p. 171). Just how Italian politicians weigh the costs of coalition in bargaining over governments remains unresolved.

Other analysts emphasize that governments are subjected to a stream of random shocks throughout their term in office. Adherents of this events approach, arguing against the school represented by Strom, object that "structural attributes do not determine the timing of the event that marks the government's dissolution" (Browne, Frendreis, and Gleiber 1988, 931; cf. Browne, Frendreis, and Gleiber 1986; Casstevens 1989; Cioffi-Revilla 1984; Ozinga, Casstevens, and Casstevens 1989; Robertson 1983). In a synthesis of the events and attributes approaches, King et al. (1990) demonstrate that, even allowing for the force of random events, some types of government—such as those in fractionalized and polarized party systems (e.g., Italy) and those with minority status—are likely to end more quickly than others (cf. Warwick and Easton 1992). This finding leads King and colleagues (1990, 869) to urge the development of micropolitical theories of cabinet dissolution. I conclude from their call that it is not very fruitful to ask whether a past government was pushed (by an unforeseeable terminal event) or jumped (at a time fixed when the government formed). The compelling questions are: How did political actors reason about a fall? How did they face—or exploit or create—the possibility of a fall? What made it more or less likely for politicians to act as they did?

In sum, anomalies appear when predictions from extant coalition theories are matched against Italian governments. A recurring theme in discussion of the anomalies is how actors think about the costs and benefits of sealing, keeping, and severing alliances. The alternative framework I propose addresses what reduces the costs of coalition, and it shows how cabinets can undergo constant change and yet remain much the same.

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[4] Following Laver and Shepsle (in particular, their analysis of Iceland in 1990a), I count parties by identifying the number holding seats in the legislature, not by calculating an index of the effective number of parties. The latter index is often used by students of coalition (e.g., Lijphart 1984).

## EXPLAINING COST REDUCTION IN COALITION BARGAINING

Coalition theories of all sorts assume that expected costs push politicians away from some behaviors, and expected benefits pull them toward others. The reasoning often runs that, in a game among parties, politicians fear loss of office or a diminished influence on policy and so are deterred from undoing governments. For a few analysts, parties, in a game with voters, pay a price for governing. The notion of costs is always implicit but is typically underdeveloped in coalition theories, for the mechanisms that may generate variations in costs are not often specified and modeled. The argument I advance thus draws from, and revises, existing studies of coalitions.

The essence of my argument is that political actors do not just see prices: They attempt to set prices attached to coalition bargaining. I assume that actors pursue gains, that they project beyond the short term, and that, if they anticipate losses, they do what they can to cut their losses. Actors will not always avoid what they identify as a costly course of action but will at times follow that course and try to lower its costs. I assume that actors face uncertainty and imperfect information. Thus, although they aim for advantage, outcomes can fall short of their intentions. I further assume that all actors care to some extent about office, policy, and votes (K. Strom 1990a). The relative priority given these objectives varies, but no political actor is utterly unmoved by the prospect of holding office, just as none is completely oblivious to policy or electoral concerns.

The game of bargaining over governments is also a game of maneuvering around or modifying the costs that coalitions entail. Political actors incur costs when they *build a coalition*. They must award ministerial portfolios to other parties, as stressed in office-driven theory. They must compromise on policy with other parties in order to come up with the government's program, as spatial theory highlights for parties outside the core. Partners in a new cabinet look ahead to the electoral benefits or burdens that governing will bring (K. Strom 1990b). As Leiserson (1968) and Axelrod (1970) reason, potential allies spend time and effort in negotiating to overcome differences. Similarly, it is costly to *sustain a coalition*. As allies govern, they divide the spoils of office, uphold old policy agreements and strike new ones, make decisions that do not please all voters, and bargain among themselves. Governing parties meet costs when *a coalition breaks apart*. At least until a new coalition emerges, the former partners may be threatened with removal from office, are handicapped in efforts to shape policy, and are open to accusations of ineffectiveness or irresponsibility, which could cost them votes. Maneuvers to destroy a coalition, unless unilateral from start to finish, carry bargaining costs as well. Thus, actors engaged in a coalition risk or incur office, policy, electoral, and bargaining costs at distinct stages in the coalition's history.[5]

I contend that political actors do not simply encounter prices but attempt to manage them. When building a coalition, actors can increase the number of portfolios; they can limit public information about policy compromises so as to ease agreement inside the coalition; and they can delegitimate opponents so as to escape voters' blame. To diminish bargaining costs, actors can devise rules to guide bargaining. Once installed, a coalition is sustained at relatively low cost if allies expand spoils and emphasize special-interest legislation. Along similar lines, actors can take steps to curtail risks when a coalition breaks up. Throughout the history of a coalition, actors can lower its costs by choosing to manipulate various levers, such as office benefits, information, reputation, and rules.

Is this sort of choice equally open and viable in all political settings? I think not. Building on existing themes in the literature (cf. Laver and Schofield 1990; K. Strom 1990a), I argue that the size and position of parties in policy space, the distribution of voters' preferences in policy space, and political institutions (in particular, electoral laws, legislative rules, and links between the executive and the legislature) affect how costly it is to break, make, and maintain coalitions and affect which cost-reduction strategies actors are likely to see as available and potentially successful. Spatial and institutional conditions are hypothesized to influence costs directly. For instance, when a government falls, a party occupying the core of policy space faces a relatively low risk of not regaining office. The anonymity afforded legislators by a secret ballot means that they can sabotage a government with little fear of losing office, antagonizing voters, or complicating bargaining. Spatial and institutional conditions are also hypothesized to influence costs indirectly, by structuring the opportunities actors have to try to lower costs. To illustrate: Office benefits are more easily increased if a core party controls government for some length of time and if preference voting directs politicians' attention to the utility of attracting personal followings through the distribution of spoils.

Another aspect of political settings conducive to cost-reduction maneuvers needs to be considered. If actors operate under uncertainty, as I assume, then cost-reduction strategies themselves pose risks. A strategy is not assured of success, and it may arouse conflict with other actors. Thus, actors are most likely to experiment with a cost-reduction strategy, first adopting or later altering it, if some change in spatial or institutional conditions brings them to recalculate the costs of coalition.[6]

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[5] The first three kinds of costs, of course, reflect the aims that each actor is assumed to have. Bargaining costs reflect the costs of acquiring and transmitting information among multiple actors.

[6] Here my reasoning resembles that of North (1990) and Tsebelis (1990) on one set of cost-reduction strategies, the redesign of institutions. I contend that institutions not only constrain politicians but also, as just noted, are subject to some degree of manipulation. Politicians are constrained by but also push against institutional constraints. This assertion leads naturally to the question: Which institutions are most

It should be stressed that, in this argument, actors incur costs. My argument responds to a puzzle about systemwide outcomes: the combination of governmental instability and stability, of short duration and restricted turnover. I do not depict those outcomes as penalties inflicted on an entire system. In some frameworks, to be sure, short cabinet duration is conceptualized as costly for the political system as a whole (e.g., Linz 1978). Here, instead, short duration is understood as a consequence of low costs (and hence of spatial and institutional conditions). If parties expect a breakup to carry low costs, they may try to hasten a government's demise. If they expect a breakup to yield benefits, they are even more likely to engineer a fall.[7] A party's exclusion from office after it has contributed to a fall (a party-specific datum that enters into the systemwide alternation rate) provides one indicator of the policy costs of cabinet dissolution. I take up this and other measures below.

To insist that actors incur costs leads to another point. Like most analysts of coalition, I think it useful to treat parties as unitary actors. Of course, parties are complex organizations, and some institutions (such as the secret ballot and preference voting) invite indiscipline. I focus on parties, however, because much of the available data pertains to them and, above all, because parties tend to join and leave coalitions as blocs (cf. Laver and Schofield 1990). I assume that parties have aims, strategies, and expectations, in brief, that they are actors as portrayed above.[8]

## Procedures and Measures in Empirical Analysis

Before evaluating this argument, three issues of research design and methods deserve discussion. The first involves the rationale for subjecting Italy to detailed examination here. Cross-national empirical research on coalitions necessarily relies on fairly large leaps in inference about the causal mechanisms linking one aggregate indicator to another. Intensive study of one country allows the analyst to move closer to decision-making processes, to sift more finely through varied sources of evidence, and to pursue traces of politicians' reasoning and calculations in ways not possible when the field of observation spans many national settings. Because the Italian record poses a stiff challenge to coalition theories, and because scientific understanding advances by assimilating anomalies into a general framework, the empirical analysis of coalition politics in this case holds special theoretical promise. Yet, it is worth repeating that Italy is extreme but not unique. Several other countries feature short-lived cabinets and limited alternation. I posit that some of the spatial and institutional conditions present in Italy also appear in those cases (and are rare or absent in still other cases, such as Ireland), and some strategies deployed in Italy should appear in those cases as well. The article's last section sketches preliminary cross-national findings that support these hypotheses.

Second, to perform the analysis, I need to measure when governments begin and end. I count a new government with each change of party composition, parliamentary election, change of prime minister, and accepted resignation of the cabinet.[9] Appendix A reports the results of applying these criteria in Italy.

Third, a test of the argument offered here plainly entails measuring the costs of coalition. To tap office costs, I count the number of ministers and undersecretaries per party in a cabinet and take averages across cabinets; figure the percentage share of all ministries and undersecretaryships each party controls; and compute the ratio between that share and the party's share of all seats in the lower house held by a government. I measure electoral costs and benefits as mean changes in parties' share of the vote between pairs of consecutive elections to the Chamber of Deputies. Policy payoffs are indexed by participation in government, which gives parties special instruments of influence on policy. Bargaining costs are captured by the time elapsed between the fall of one government and the rise of its successor. Further details on measures appear in the analysis below.

## ASSESSING THE COSTS OF COALITION IN ITALY

The Italian-specific version of the general argument is that particular spatial and institutional conditions in Italy lower the costs associated with breaking and building coalitions and favor strategies that further lower those costs. Italy's coalitions are not easily sustained, since breakups cause little damage. Because this logic is put into practice, I claim, Italy displays ephemeral governments but permanent incumbents. The Italian pattern is personified in Giulio Andreotti, a Christian Democrat who held posts in 36 governments between 1947 and 1992, including seven turns as prime minister.

My test of these assertions proceeds in two steps. I first look for evidence of behavior that could be inter-

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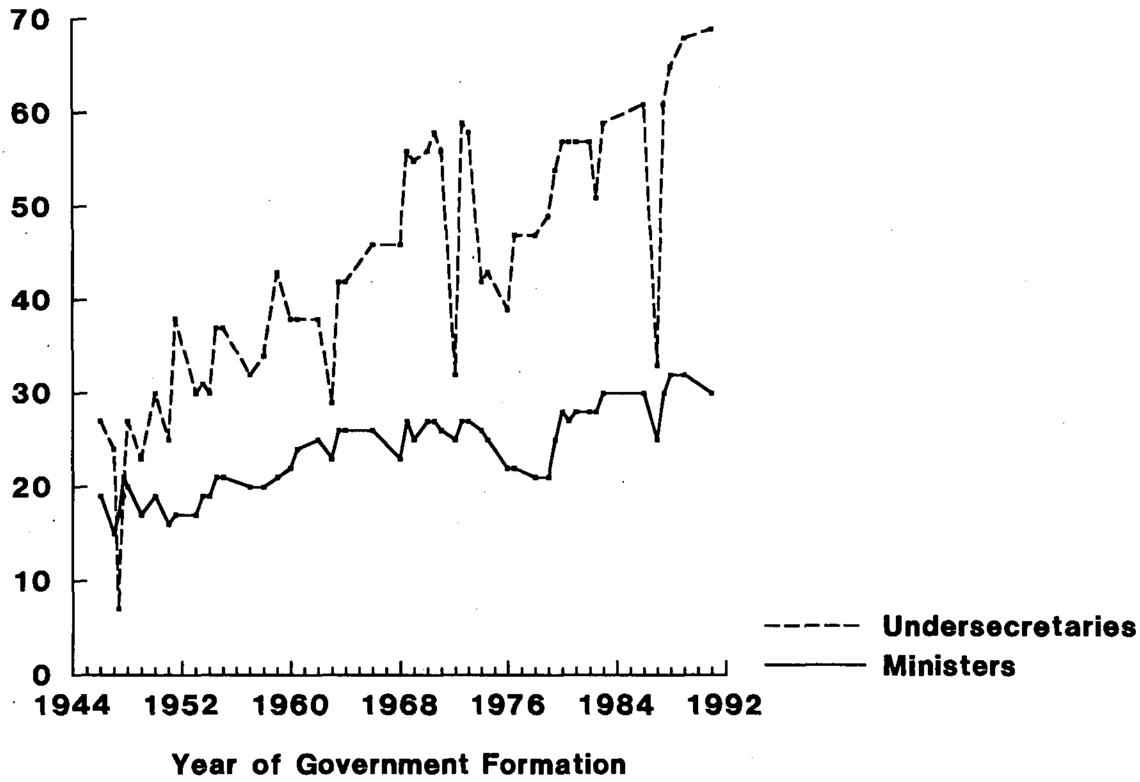
usefully viewed as exogenously fixed and which are within the power of the actors to change? Of the three sets of rules emphasized here, links between the executive and the legislature (in particular, the vote of investiture) arguably have the deepest constitutional import; at the other extreme, parliamentary procedures (such as use of the secret ballot) tend not to have constitutional import. Many democracies impose supramajoritarian decision rules on constitutional changes and require only pluralities for changes in parliamentary procedure (Herman 1976). In ongoing research I examine long-run change more fully. See Knight 1992.

[7] Hence, low or negative expected costs (and spatial and institutional conditions) produce short durations. Over time, however, as short-lived governments repeatedly collapse at low cost, they are likely to induce expectations among politicians that attempts to reduce costs further will succeed. In the long run, then, short durations encourage cost-reduction strategies and produce low(er) costs. An empirical estimate of these feedback effects lies beyond the scope of this article. I am exploring such effects in cross-national research. See King, Keohane, and Verba 1994, 185–96.

[8] I relax the unitary actor assumption and study factions in ongoing research. This step is especially important for Italy, since many scholars highlight the role of factions in Italian coalition politics (e.g., Dogan 1984; Mershon 1994; Zuckerman 1979).

[9] These criteria replicate those used for Italy by K. Strom (1985, 1990b) and others who have borrowed Strom's data (King et al. 1990; Warwick and Easton 1992). I differ from Laver and Schofield (1990), who use only the first two criteria. By each set of criteria, Italy has the most short-lived governments of any extant parliamentary democracy.

FIGURE 3. Ministers and Undersecretaries in Italian Governments, 1946–92



preted as attempts to reduce the costs of coalition. I next investigate whether spatial and institutional conditions do in fact influence costs and cost-reduction strategies in Italy.

## The Pursuit of Cost-Reduction Strategies

I expect to find that Italian politicians have actually handled such tools as office benefits, information, and reputation in efforts to curb the costs of coalition. In what follows I review evidence from the historical record that fulfills this expectation (for details, see Mershon n.d.).

*Expansion of Offices.* The number of posts in Italian governments has increased since 1946. As Figure 3 shows, declines have occasionally interrupted the overall rise, but the total of cabinet members has never returned to the level characteristic of the late 1940s. Politicians have enlarged spoils outside the cabinet as well, expanding and penetrating the already sizable state bureaucracy and public sector inherited from Fascism (Di Palma 1979; Tarrow 1990).

*Restriction of Information.* Italy's Constitution requires new governments to present a program to Parliament. Most programs are "grandiose enough to impress public opinion and ambiguous enough to be accepted by every [party] delegation" in the government (Marradi 1982, 65). No government has ever been forced to resign by the Parliament's approval of a motion of no confidence (Manzella 1991, 306–8). Why? According to Balboni (1988, 49), "the [same] allies have historically always been obliged to govern together: It would not be advisable to accentuate contrasts necessarily destined to find some settlement."

*Delegitimation of Opponents.* For more than forty years, parties at or near centrist positions have defined the PCI and the neo-Fascist Italian Social Movement [Movimento Sociale Italiano (MSI)] as unacceptable allies, outside the circle of legitimate contenders for cabinet posts.[10] The centrist parties have publicized this rejection and ideological justifications for it. Yet, nonideological benefits have flowed from delegitimation of opponents. Since the delegitimated parties have been relatively large (commanding 30–40% of seats in the Chamber of Deputies), governments have fallen without risking full alternation in power (cf. Sartori 1976).

*Expansion of Coalitions.* Italian politicians have typically sought surplus coalitions (K. Strom 1990b, 144). The DC always governed with other parties between 1948 and 1953, the only time it held a majority in the Chamber of Deputies. Italy's few minimal winning coalitions have received external support on their votes of investiture. All but four minority governments have been buttressed by external parliamentary support, and all but one have formed after multiple attempts. Minorities have governed as temporary conveniences during phases of exceptional discord among the numerous usual allies (Zuckerman 1979, 152–3). What good are oversized governments? Surplus status has guarded legislation against wayward parliamentarians from the governing majority who have voted against government bills behind the mask of the secret ballot. Studies of electoral behavior in Italy and other cases find that "increasing the number of parties in government weakens the economically inspired, anti-government vote," a result attributed to the "diffusion of government responsibility" that large coalitions permit (Lewis-Beck 1986, 341, 342).

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[10] The PCI had cabinet ministries from 1944 to 1947, when governments were based on cooperation in the anti-Fascist Resistance. In May 1947, with East-West tensions turning into the Cold War, the DC forced the PCI and PSI out of government. As I discuss below, the renamed MSI entered government in May 1994.

***Reliance on Rules: Coalition Formulae.*** The repeated inclusion of certain parties in surplus coalitions and the repeated exclusion of other parties have added up to the following patterns in cabinet composition. Under centrism (1947 to 1963), the DC allied with the Social Democrats [Partito Socialista Democratico Italiano (PSDI)], Republicans [Partito Repubblicano Italiano (PRI)], and Liberals [Partito Liberale Italiano (PLI)]. In center-left coalitions (1963 to 1976), the Socialists replaced the Liberals in governing with the DC, PSDI, and PRI. Under national solidarity (1976 to 1979), the Communists joined other parties in supporting minority DC governments through parliamentary accords. And in five-party (*pentapartito*) coalitions (1979 to the 1992 elections), both Socialists and Liberals governed with the DC, PSDI, and PRI. Italians use the term *formulae* to refer to these coalition designs. The formulae are not simply regularities in outcomes but are also constraints on the process of coalition formation, limits that centrist politicians have put on their own behavior (Mershon 1994). These informal rules, which prescribe customary governing partners and proscribe unacceptable contenders for governing power, narrow the range of unsettled issues in coalition bargaining. The formulae also codify and perpetuate expectations that politicians who undo one cabinet will be able to enter negotiations for the next cabinet and try to improve on portfolio assignments or policy influence. The stability of coalition formulae fosters cabinet instability.[11]

***Revision of Rules.*** Incumbents have altered formal and informal rules in hopes of counteracting electoral losses. The centrist parties suffered sizable losses in the local elections of 1951 and 1952. Shortly before the 1953 parliamentary elections, the DC secured the passage of a controversial law that would have given 65% of seats in the Chamber of Deputies to an alliance of parties that received barely half the votes cast. The law never took effect, for the 1953 elections awarded only 49.2% of the vote to the centrist alliance; the DC lost the parliamentary majority it had won in 1948; and the law was rescinded soon after. Hence, the majoritarian law not only failed to diminish the costs of setting up and sustaining governments but also exacted a substantial price. The DC's allies in centrism had new leverage, for the DC now needed their support to pass votes of investiture and approve legislation.

Faced with weaknesses in the centrist coalition, the Christian Democrats moved toward adjusting the coalition formula. A center-left coalition became an admissible option for growing segments of the DC after the PSI renounced unity of action with the PCI in 1956. The PSI's return to government, realized in 1963, was intended to isolate the PCI and win the government greater working-class support (Tamburrano 1971).

***Codification of Bargaining Procedures.*** In summer 1968, a prominent Christian Democrat and his aide began to develop systematic guidelines for bargaining over new governments. The basic principle was that shares of ministries should follow the relative strengths of factions within the DC and of allied parties. This principle conformed to what had already been established as common practice in 1953, due to the failure of the majoritarian electoral law (Dogan 1984, 170). But the standard procedure was extended and routinized immediately after the 1968 elections. In a series of memoranda, DC cabinet member Adolfo Sarti and his aide, Massimiliano Cencelli, calculated factional strength on the DC National Council down to hundredths of percentages and updated measurements between DC congresses (Venditti 1981, Appendix 1); they dealt with qualitative differences among cabinet posts, identifying four tiers from top to bottom (pp. 112–3); they weighed the post of prime minister as equal to two ministerial portfolios, and one ministerial portfolio was equal to three undersecretaries (Dogan 1984, 169–72). This codification of procedures, known as "the Cencelli manual," saved on bargaining costs. It has structured coalition negotiations so frequently and so thoroughly in Italy since 1968 that one politician has quipped that the manual amounts to "the true Constitution of the Republic" (a former president of the Liberal Party, quoted in Venditti 1981, 50). One journalist has documented use of the Cencelli manual for all government formations between 1968 and summer 1981 (Venditti 1981). According to Cazzola (1985, 202), the manual lies "at the base of all [coalition] bargaining" in Italy.[12]

***Special-Interest Legislation.*** A central theme in studies of the Italian Parliament is that its legislation is both voluminous and trivial (cross-national data in Di Palma 1977, 41–2, 48, 69; cf. Guarnieri 1989). By focusing on special-interest legislation, parliamentarians have located common ground for policy compromise, facilitated bargaining, and so cut the price of maintaining governments. The costs of dismantling governments have also decreased, since politicians have reason to hope that they can avert electoral punishment by servicing targeted constituencies.

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[11] Obviously, the existence of four formulae indicates that they are not impervious to change. I treat the shift from centrism to center-left coalitions below. On other changes, see Mershon 1994.

[12] Disputes over recent implementation of the guidelines (in negotiations over Andreotti VII) appear in *Corriere della Sera*, April 15 and 21, 1991. Below, I discuss the 1995 Dini government, which abandoned the manual.

## The Effect of Spatial and Institutional Conditions on Costs and Cost-Reduction Strategies

The evidence marshaled so far points to a clear conclusion: Italian politicians have attempted to lower the costs of coalition by a variety of means. The other major component of the argument must now be assessed: whether spatial and institutional conditions have aided politicians in their efforts. If those conditions do influence costs and cost-reduction strategies, then parties should differ in the costs they incur and the strategies they implement. And if those conditions shift over time, then costs and strategies also should show some change. Events consistent with this logic have already been observed. For instance, the PSI's entry into government, followed by a reranking of winners and losers in the 1968 elections, prompted the drafting of written guides to bargaining; and the authors of the Cencelli manual came from Italy's core party. To extend this analysis, I first outline posited influences on electoral costs and trace electoral costs incurred. I then turn to the office, policy, and bargaining costs attached to coalitions.

***Voters' Preferences.*** Religion and class have powerfully shaped voters' choices in postwar Italy (e.g., Barnes 1977; Galli and Prandi 1970). Hence, a six-nation comparison finds that "only in Italy do changing social cleavages [religion and class] have more of an effect than changing economic evaluations" on the probability of voting for incumbents (Lewis-Beck 1988, 157). Such limits on rational policy voting inform a well-known typology of Italian electoral behavior (Parisi and Pasquino 1979), which distinguishes a vote of opinion, motivated by a broad interest in policy; a vote of belonging, affirming an enduring allegiance to either the Catholic or Communist subculture; and a vote of exchange, awarded in return for patronage goods. Mannheimer and Sani (1987, 93) use a 1985 survey to estimate that subcultural voters constitute 60% of the DC electorate and 67% of the PCI electorate. Other data suggest that exchange voters form sizable proportions of the DC and PSDI electorates, that opinion voters predominate in the PLI and PRI electorates, and that the PSI (especially since the early 1960s) draws opinion and exchange voters (e.g., Caciagli 1988; Cazzola 1985; Katz 1985). It is accepted wisdom that socioeconomic change has recently eroded the subcultural vote and augmented the opinion vote in Italy.

***Electoral Laws.*** As Appendix B indicates, from 1946 until 1993 a relatively pure version of proportional representation made a place for small parties in the Italian Parliament and assigned a small bonus of parliamentary seats to large parties. It is often claimed that preference voting has encouraged factionalism in Italian parties (Katz 1986; Zuckerman 1979). As one manual for political candidates cautions, intraparty competition has been at least as fierce as interparty competition during election campaigns (Pustetto 1991, 35–6, 79–81).[13]

***The Sizes and Positions of Parties in Policy Space.*** Content analyses of party manifestos show two leading dimensions of party competition in Italy: a socioeconomic left-right dimension and a technocracy-social harmony dimension subsuming Catholicism (Mastropaolo and Slater 1987). The array and strength of parties in this policy space established the DC as a core party from 1946 through 1992. The 1976 elections made the PSI pivotal, for it became the essential ally in any DC-based coalition capable of commanding a majority while excluding the PCI (Pasquino 1981). The DC's sizable losses in the 1992 elections ended its status as a core party (Schofield 1993); and in 1992 the PSI ceased to be pivotal.

***The Electoral Costs of Building and Sustaining Coalitions.*** Italy's governing parties have suffered smaller electoral losses, on average, than have governing parties in many other democracies (K. Strom 1990b, 124, 181–2). Like Strom, I measure electoral costs as mean shifts in vote shares between pairs of consecutive elections to the lower house of parliament. To explore how spatial and institutional conditions have influenced the electoral fortunes of incumbents in Italy, Table 1 disaggregates the analysis by party and distinguishes between a party's presence in office on election eve and its role in all governments formed from one election to the next.[14]

Electoral payoffs for all governing parties in Italy have depended on spatial conditions and the segmentation of the electorate. An incumbent party for five decades, the DC scored slight electoral gains when it governed alone on election eve (an increase of 0.8% of the vote, on average, as listed in the first column of Table 1). The DC lost the least votes (a mere 0.1%, on average) when it governed with allies for most of the time between elections. This two-part finding supports the notion that exchange voters respond strongly to spoils delivered by a single governing party and that multiparty governments blur responsibility and spread blame. A third item deserves notice, however: the losses that the DC posted in coalition from 1976 to 1992. The DC became more vulnerable with the erosion of the subcultural vote, the rise in opinion voting, the more intense interparty competition for spoils growing out of the PSI's pivotal position, and less favorable conditions for the ideological delegitimation of opponents.

As Table 1 displays, the DC's habitual allies—with one exception—received more votes when they held opposition, rather than governing, status immediately before elections. The Social Democratic exception reflected its pool of exchange voters (cf. K. Strom 1990b, 172). The PRI benefited more from preelectoral opposition than did the PLI, for its more centrist position allowed the PRI to attract more opinion voters dissatis-

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[13] Amid mounting criticism of just these effects, a 1991 referendum restricted preference voting in Italy. Below I briefly discuss the effect of this change, as well as the radical overhaul of electoral laws that Parliament approved in 1993. On factions, see note 8 above.

[14] The distinction is most useful after 1968, for since then (save in 1992) very short-lived cabinets have ushered in early elections. Strom and I use the same measure of electoral performance but different units of analysis, for Strom examines the set of incumbent parties in the aggregate. Thus, my analysis extends Strom's work on electoral costs (1990b, 123–5, 165–76).

**TABLE 1. Electoral Gain or Loss, by Party and Government Status, 1948–92**

| | DC | PSI | PSDI | PRI | PLI |
|---|---|---|---|---|---|
| Status Immediately Before Election | | | | | |
| One-party government | +0.80 (*n* = 4) | NA | NA | NA | NA |
| Coalition government | −1.23 (*n* = 7) | −0.93 (*n* = 3) | +0.04 (*n* = 6) | −0.46 (*n* = 5) | −0.43 (*n* = 3) |
| Opposition | NA | −0.61 (*n* = 8) | −0.92 (*n* = 5) | +0.38 (*n* = 6) | −0.34 (*n* = 8) |
| Dominant Status Between Elections | | | | | |
| One-party government | −2.20 (*n* = 2) | NA | NA | NA | NA |
| Coalition government | −0.11 (*n* = 9) | −0.08 (*n* = 5) | −0.79 (*n* = 9) | +0.22 (*n* = 5) | +0.35 (*n* = 4) |
| Opposition | NA | −1.34 (*n* = 6) | +0.95 (*n* = 2) | −0.18 (*n* = 6) | −0.77 (*n* = 7) |
| Coalition govt. '76–'92 | −2.87 (*n* = 3) | +1.27 (*n* = 3) | −0.37 (*n* = 3) | +0.47 (*n* = 3) | +0.30 (*n* = 3) |

*Source*: Calculations based on government status data reported in Appendix A and electoral data in Appendix B.
*Note*: Entries are mean changes in parties' percentage point share of the vote between pairs of consecutive elections to the Chamber of Deputies. Numbers of applicable interelectoral periods are in parentheses. The baseline year is 1946 for all parties except the PSDI, which first contested elections in 1948. Dominant status is coded as whatever status (in one-party government, in coalition government, in opposition) was most frequent between two successive elections.
NA: Not applicable.

fied with government policy (Mannheimer and Sani 1987, 168). The costs of coalition on election eve tended to be offset by advantages accruing from longer term governing roles. That is, the PSI, PRI, and PLI performed better when they shared governing power for most of the span since the previous election, rather than joined the opposition. The leftmost and rightmost governing parties, the PSI and PLI, suffered more from interelectoral opposition than did the PSDI and PRI and spent more time in opposition. The PSDI operated in opposition even less than did the PRI; and when the PSDI's dominant interelectoral status was opposition, it campaigned on its prospective role in coalitions (Galli and Prandi 1970, 39–42; Leonardi 1981). From 1976 to 1992, all but one of the DC's allies reaped electoral benefits from participation in coalitions. The gains were greatest for the PSI, whose rightward moves captured opinion voters and whose pivotal contributions to DC-based majorities helped it tap and deliver spoils. Overall, then, the parties prescribed as the DC's allies under coalition formulae usually won electoral credit for brief exits from coalition and banked on reentering government, looking for additional credit.

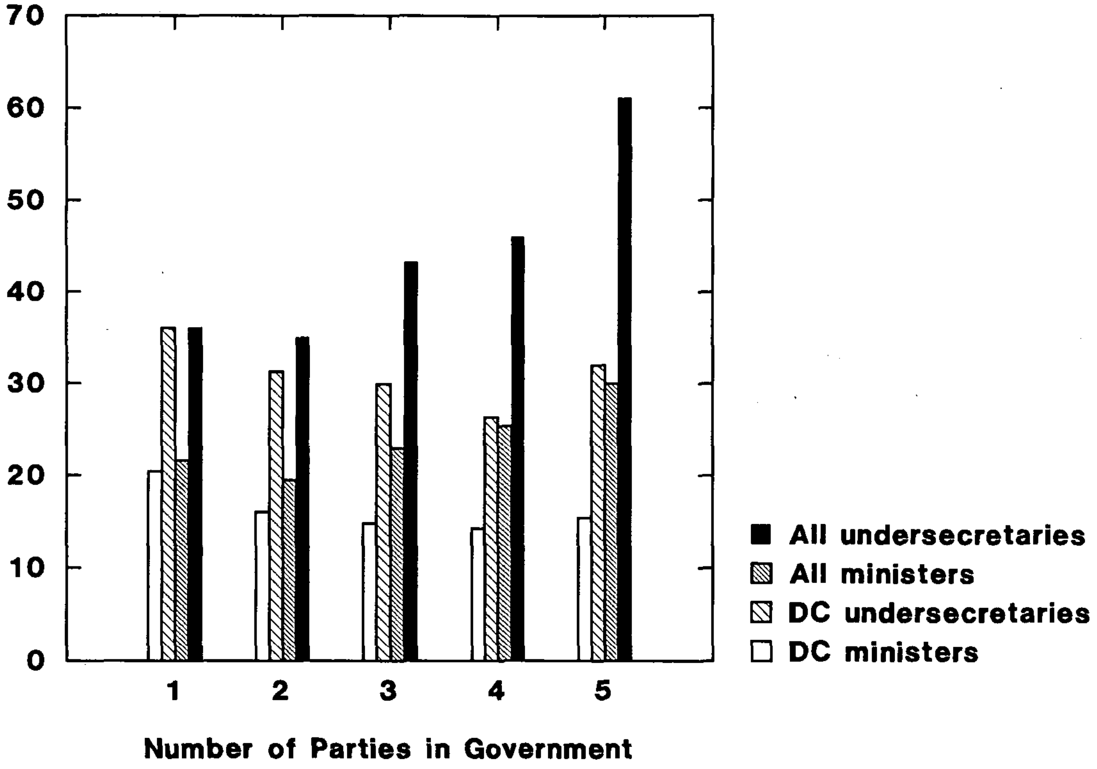
***The Office Costs of Building Coalitions and Strategies for Managing Office Costs.*** Table 2 measures the office price that the DC paid when it governed with coalition partners. The top row of each subtable A and B reports the mean percentage of portfolios held by the DC in different types of governments. As the first column indicates, in one-party governments the DC occasionally awarded a few posts to independent experts. In coali-

**TABLE 2. Office Payoffs (Ministers and Undersecretaries) by Type of Government, 1946–92**

| Office Payoffs | One-Party Governments (N = 16) | All Coalitions (N = 36) | Coalitions Including PSI (N = 22) | 1976–92 Coalitions Including PSI (N = 11) |
|---|---|---|---|---|
| A. Mean share of all ministers[a] | | | | |
| Christian Democrats (DC) | 94.4% | 61.0% | 55.3% | 52.1% |
| Non-DC[b] | 5.6 | 39.0 | 44.7 | 47.9 |
| Total | 100.0% | 100.0% | 100.0% | 100.0% |
| Mean total N of ministers | (22) | (25) | (27) | (29) |
| B. Mean share of all undersecretaries[a] | | | | |
| DC | 99.1% | 64.2% | 55.3% | 54.2% |
| Non-DC[b] | 0.9 | 35.8 | 44.7 | 45.8 |
| Total | 100.0% | 100.0% | 100.0% | 100.0% |
| Mean total N of undersecretaries | (36) | (47) | (53) | (60) |
| C. Mean ratios[c] of cabinet post shares to government seat shares[d] for DC | | | | |
| Ministers | .94 | .82 | .83 | .78 |
| Undersecretaries | .99 | .85 | .83 | .81 |
| D. Mean number of posts held by DC | | | | |
| Ministers | 20 | 15 | 15 | 15 |
| Undersecretaries | 36 | 29 | 29 | 33 |

*Source*: Calculations based on portfolio data in Petracca 1980 and *Corriere della Sera*, various issues, and seat data in Appendix B.
[a] Entries are mean percentage shares for all governments of the type listed at the head of each column.
[b] Includes nonparty experts and politicians from parties other than the DC.
[c] Entries are mean ratios for all governments of the type listed at the head of each column.
[d] Government seat share is defined as a party's percentage share of the seats in the Chamber that are controlled by all governing parties.

FIGURE 4. Office Payoffs, by Number of Parties in Italian Government, 1946–92 (means)



tions (shown in the second column), the DC sacrificed this near monopoly on cabinet slots. The third and fourth columns disclose that the DC relinquished a greater percentage of offices when the coalition embraced the medium-sized PSI and still more in the 1976–92 coalitions when the PSI made a pivotal contribution to the governing majority.

Subtable 2C establishes that the division of portfolios between the DC and its allies illustrates a well-known finding from cross-national portfolio studies: The largest party in a coalition obtains a share of senior cabinet posts that is somewhat smaller than that party's share of the parliamentary seats controlled by the government (Browne and Franklin 1973; Laver and Schofield 1990, chapter 6; Schofield and Laver 1985). The disadvantage that the DC met was only slightly less pronounced for shares of undersecretary posts. Intervals of one-party government thus compensated the DC for disproportionate shares of portfolios surrendered to allies at other times (cf. Marradi 1982).

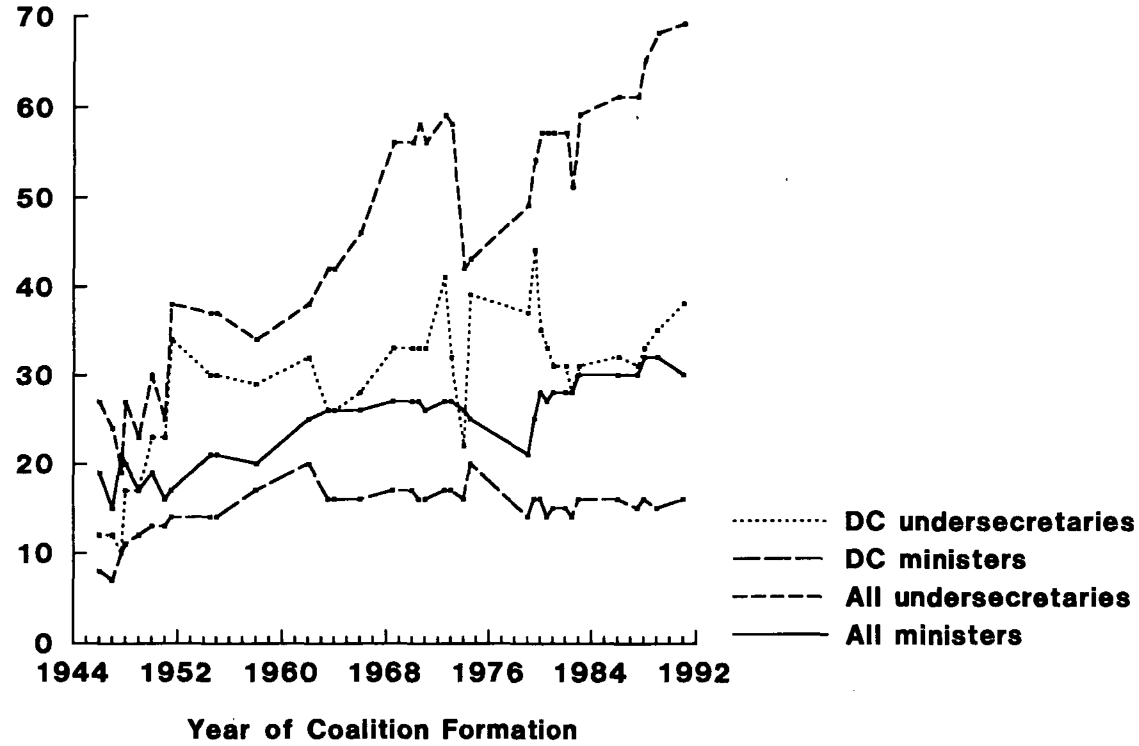
Subtable 2D spotlights average numbers of offices. In these terms, the DC spent less to construct coalitions than might be expected—and much less than office-driven theory would predict. On average, it cost Italy's core party *nothing* in number of cabinet slots to include the PSI in a coalition, even when the PSI was pivotal. Indeed, DC undersecretaries were *more* numerous in coalitions containing a pivotal PSI than in other types of coalitions.

As Figure 4 reveals, portfolio inflation was the cost-management strategy that produced these outcomes. Offices, especially undersecretaryships, were like balloons, inflated when needs arose. The number of allies changed, but the number of Christian Democrats in government remained remarkably stable.

The distinction between ministers and undersecretaries is worth weighing. As coalitions expanded from two parties to three, four, and then five parties, the number of ministers was pumped up steadily, the number of undersecretaries rather unevenly. The largest boosts in undersecretaries separated two- from three-party coalitions and four- from five-party coalitions. Why? None of Italy's two-party coalitions included the PSI, and all five-party coalitions governed when the PSI was pivotal. The competition for office between the DC and the PSI led to mutual accommodation and thus drove portfolio inflation.

As Figure 5 plots, the steepest increases in ministerial and undersecretary posts occurred when the PSI reentered government after a sojourn in the opposition (the early 1960s, when center-left coalitions were prepared and implemented, and the early 1980s, when five-party coalitions were instituted). Those hikes enabled the DC to protect or even add to its portfolios. Why have undersecretaryships been more elastic balloons than senior cabinet posts? Undersecretaries are arguably less visible to voters and imply a less permanent, more flexible commitment of resources. Moreover, in a long-standing "informal division of ministerial labour, [an undersecretary] is left free to distribute the patronage of the ministry in his constituency" (Allum 1973, 90). The secret ballot made it "necessary . . . to satisfy the

FIGURE 5. Office Payoffs in Italian Coalitions, 1946–92



greatest possible number of deputies," and coalition builders used undersecretary posts as a convenient currency with which to gratify deputies and buy loyalty (Dogan 1984, 164; cf. Venditti 1981). Since parliamentary rules require that members of government—both ministers and undersecretaries—step down from parliamentary committees (Nocifero and Valdini 1992, 70), an increase in cabinet posts has the advantage of enlarging access to committee positions for parliamentarians without cabinet responsibilities. Members of multiple committees are well equipped to pipe narrow benefits to clienteles.

Before turning to parliamentary rules and committees, I should confront a question posed by the record of portfolio inflation: Is the aggregate value of the cabinet more aptly viewed as fixed or variable? If politicians regard the value of the cabinet as fixed (so that creating new posts dilutes the worth of old ones), then they should express their counts of offices in percentage terms. If they see the value as variable, then their counts should in some way convey increases in cabinet value with new portfolios. Published notes from coalition bargaining (Venditti 1981) show that Italian politicians focus on both cabinet shares and the value added by new posts. Especially revealing is a memorandum from 1970, which lists in one column the numbers of undersecretaries that the sitting government (Rumor III) would have owed each DC faction if the allotment of offices had mirrored the shares of seats held by factions on the DC National Council. In the next column appear, faction by faction, the 33 DC undersecretaries actually serving in Rumor III. Still other columns report proportional interfactional allocations for hypothetical scenarios involving 48, 50, and 55 DC undersecretaries. With 48 DC undersecretaries, one faction underrepresented in Rumor III would have been able to boost its undersecretaries (the faction headed by Paolo Emilio Taviani, whose allocation would have jumped from two to five) without subtracting posts from a rival faction overrepresented in Rumor III (whose undersecretaries would have remained at six). This example suggests that the use of two yardsticks is not so much inconsistent as expedient: The authors of the memorandum were major figures in the Taviani faction.[15]

***Parliamentary Rules.*** The Italian Parliament has standing, specialized committees whose powers to take final action on legislation make them "miniparliaments" (Hellman 1992, 373). The committees have handled the bulk of legislation—about 63% of the legislative proposals in a 1963–72 sample and 54% of enacted laws from 1987 to 1992 (Di Palma 1977, 201; Nocifero and Valdini 1992, 11). Both within and outside committees, governing parties have typically sought to avert upsets by including the major opposition parties in the preparation of legislative proposals. This practice has furnished a defense against the secret ballot, which was required on final votes in the Chamber of Deputies until 1988, and has given shelter to governing parties' parliamentarians who have voted against government bills.

***Links between the Executive and the Legislature.*** According to a noted constitutional lawyer, "no modern parliament raises so many negative conditions for a

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[15] Obviously, the example also illustrates the interest of studying factional politics (see note 8).

**TABLE 3. Electoral Gain or Loss, by Party and Responsibility for Government Collapse, 1948–92**

| Dominant Role in Collapses Between Elections | DC | PSI | PSDI | PRI | PLI |
|---|---|---|---|---|---|
| Responsible | +0.30 (*n* = 10) | −0.03 (*n* = 8) | −0.50 (*n* = 4) | −0.10 (*n* = 4) | −0.80 (*n* = 1) |
| Not responsible | −8.40 (*n* = 1) | −1.73 (*n* = 3) | −0.40 (*n* = 7) | +0.06 (*n* = 7) | −0.32 (*n* = 10) |
| Responsible 1976–92 | −2.25 (*n* = 4) | +1.00 (*n* = 4) | −1.10 (*n* = 1) | −0.75 (*n* = 2) | −0.80 (*n* = 1) |

*Source*: Calculations based on coding of information in *Keesing's Contemporary Archives* and electoral data in Appendix B.
*Note*: Entries are mean changes in parties' percentage share of the vote between pairs of consecutive elections to the Chambers of Deputies. Numbers of applicable interelectoral periods are in parentheses. The baseline year is 1946 for all parties except the PSDI, which first contested elections in 1948. Dominant role is coded as whatever role (responsible or not responsible for collapse) was most frequent between two successive elections.

government; no government is weaker in parliament than the Italian" (Andrea Manzella, quoted in Spotts and Weiser 1986, 111). One negative condition is the vote of investiture. The Constitution stipulates that a government takes office when its ministers swear loyalty to the Republic before the president, but it further requires that the new government submit to a vote of confidence in both chambers of Parliament within ten days of its inauguration. If a government fails on either vote, it must resign, a fate met by five governments to date. Governments that pass the votes of investiture meet other obstacles. No rule has ever assigned the government direct responsibility for setting the legislative calendar. Until 1971, there were no ceilings in either chamber on the number of bills that private members could propose (Baldassare 1985). Governments can attempt to compel parliamentary majorities to emerge through the provision for executive decrees, which expire unless enacted by Parliament within sixty days of their issue.

To assess how the rules just reviewed have influenced the costs of breaking coalitions, I again disaggregate by party and track trends over time. I also identify which parties were responsible for cabinet collapses.[16]

***The Electoral Costs of Destroying Governments.*** As before, I measure electoral costs as mean shifts in shares of the vote between pairs of consecutive elections. My concern now is to ascertain how a party fares at the polls after it has contributed to the fall of most governments formed in the span between two elections. Of course, factors other than a party's role in provoking or preventing cabinet dissolutions—such as levels of inflation and unemployment—can bring voters to penalize or reward incumbent parties (e.g., Eulau and Lewis-Beck 1985; Lewis-Beck 1988; Powell and Whitten 1993). I simply wish to observe whether parties that have caused cabinet failures encounter electoral punishment.

Until 1992, the Christian Democrats and Socialists benefited electorally from knocking down governments. Yet, as Table 3 shows, distinctions separated the DC and PSI in this regard.[17] The top row of the table discloses that the DC gained at the polls when it helped upset most cabinets between two successive elections (advancing 0.3%, on average), whereas the PSI contained its electoral losses when it toppled governments (slipping only 0.03%, on average). The bottom row of Table 3 isolates the years when the PSI held pivotal status. From 1976 to 1992, the PSI won votes despite, or due to, its responsibility for terminating every government launched after 1974. The DC, in contrast, suffered for its role in cabinet falls after 1976.

The electoral payoffs for demolishing governments, like those for erecting governments, have reflected spatial conditions and the segmentation of the electorate. For decades the DC undid governments with impunity, since loyal subcultural voters long dominated the DC electorate, and exchange voters long expected a continuing flow of spoils from the large, centrally located DC.[18] Cabinet dissolutions cost the DC votes after 1976 because, as noted earlier, subcultural voting waned, opinion voting increased, the pivotal PSI put up stiffer competition for spoils, and ideological justifications for delegitimating opponents had less force. After 1976, furthermore, responsibility for breakups became more of an electoral liability for the PSDI and PRI and more of an electoral advantage for the PSI. Some voters seem to have defected in response to perceived disruptions of policy (opinion voters prevalent within the PRI electorate), others in response to potential complications in patronage (the PSDI's exchange voters). Still other voters rewarded the pivotal PSI for its capacity to influence policy and tap spoils, a capacity exhibited and exploited in government falls (cf. Mannheimer and Sani 1987, 167–82). The rightmost PLI was exposed to relatively severe electoral punishment for dismantling cabinets, which helps explain why it risked punishment rarely. On the whole, however, these differences across parties were small. No party courted electoral disaster when it extinguished a government.

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[16] This identification relies primarily on *Keesing's Contemporary Archives* (1945–92), a source often used in coalition studies (e.g., Budge and Keman 1990; Dodd 1976). Designation of responsibility for a cabinet's fall was generally straightforward. For example, in February 1960 the PLI National Council voted to withdraw parliamentary support from the government. Three days later Premier Antonio Segni submitted his resignation. Responsibility for that fall lay with the Liberals. Even when a cabinet has resigned after defeat on a secret ballot, insiders have speculated to journalists about the vote and estimated how many deputies from each governing party defected from the governing majority (*L'Espresso*, July 6, 1986, 9, 11).

[17] Table 3 does not analyze responsibility for collapses immediately before elections because that variable varies so little: Before 1968, no party was ever responsible for the fall of a cabinet on election eve.

[18] The DC's drop when not responsible for cabinet collapses rests on one datum alone: the difference between 1948, when voters rallied around the DC at the onset of the Cold War, and 1953, when controversy over the majoritarian electoral law repulsed some voters.

**TABLE 4. Office Payoffs, by Party and Responsibility for Government Collapse, 1946–92**

| | DC | | PSI | | PSDI | | PRI | | PLI | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Was Party Responsible for Collapse at Time *t* − 1? | | | | | | | | | |
| | no | yes | no | yes | no | yes | no | yes | no | yes |
| | Ratios of cabinet post shares to government seat shares[a] (means) | | | | | | | | | |
| Offices at *t* | | | | | | | | | | |
| Party ministers | 0.89 | 0.83 | 1.30 | 1.23 | 1.84 | 1.84 | 3.00 | 2.25 | 2.09 | 2.12 |
| Party undersecretaries | 0.91 | 0.89 | 1.39 | 1.34 | 1.70 | 1.53 | 1.85 | 1.55 | 2.16 | 1.85 |
| N cabinets | (21) | (30) | (3) | (18) | (21) | (8) | (17) | (8) | (12) | (4) |
| | Number of posts (means) | | | | | | | | | |
| All ministers | 23 | 24 | 21 | 25** | 24 | 23 | 23 | 25 | 24 | 24 |
| All undersecretaries | 42 | 45 | 35 | 49** | 44 | 44 | 41 | 48 | 44 | 44 |
| N cabinets | (21) | (30) | (20) | (31) | (33) | (18) | (35) | (16) | (43) | (8) |
| | Number of posts 1976–92 (means) | | | | | | | | | |
| All ministers | 26 | 30* | NA | 27 | 27 | 30 | 28 | 26 | 27 | 28 |
| All undersecretaries | 53 | 62* | NA | 56 | 55 | 61 | 55 | 56 | 55 | 59 |
| N cabinets | (5) | (11) | (0) | (16) | (14) | (2) | (11) | (5) | (11) | (3) |

*Source*: Calculations based on portfolio data in Petracca 1980 and *Corriere della Sera*, various issues, and seat data in Appendix B; and coding of information in *Keesing's Contemporary Archives*.

*Note*: The top two rows of entries report, for each party listed, mean ratios of cabinet post shares to government seat shares. The other rows report means of total cabinet posts. Means are computed, party by party, for two groups of cabinets: those whose predecessor was a cabinet the party helped to topple and those whose predecessor fell for reasons other than the party's withdrawal of support. *T* tests compare the means of the two groups (i.e., for each party, entries in the "yes" and "no" columns.) Numbers of applicable cabinets are in parentheses.

NA: Not applicable

[a] government seat share = a party's share of the Chamber of Deputies seats controlled by all governing parties.

* $p < .05$ (separate variances *t* test)

** $p < .001$ (separate variances *t* test)

***The Office and Policy Costs of Destroying Governments and Strategies for Managing Costs.*** As typically measured, the office price of ending governments is negligible in Italy. As Table 4 displays, the ratio of the DC's share of cabinet ministers to its share of the government's parliamentary delegation averaged 0.83 when the DC had contributed to demise of the preceding cabinet. This figure diverged hardly at all from the mean weighted share of ministerial posts (0.89) that the DC acquired when the party had done nothing to undermine the previous executive. More broadly, all five governing parties received a weighted share of senior cabinet posts that stayed roughly steady, on average, whether or not they had overthrown the preceding government. The same statement holds true for weighted shares of undersecretary posts. Not a single pair of ratios in the top two rows of Table 4 is significantly different. This finding runs counter to conventional assumptions about the office costs of breakups. It becomes even more remarkable when average numbers are considered. The total of cabinet posts varied little according to whether the PSDI, PRI, or PLI had pulled down the prior government. But a government's ministers and undersecretaries, on average, were significantly *more* numerous after the Socialists had sabotaged, as compared to sustained, its predecessor. With the PSI's percentage of offices unaltered and the number of all offices up, more Socialists filled cabinet slots. Office *benefits* went to the PSI after it had eliminated a government, above all when the PSI was pivotal. Along similar lines, but only from 1976 to 1992, a constant share and a significantly larger total of offices meant that more Christian Democrats attained cabinet positions after the DC had toppled a government. In this sense, too, the competition between the DC and PSI drove portfolio inflation.

Membership in government offers parties special opportunities to influence policy. Hence, Table 5 examines policy benefits at risk by designating the governing status of parties behind government falls. It shows the policy payoffs by reporting how often parties that bring about one government's demise participate in the next government and produce a change in incumbent parties.[19] Subtable 5A illustrates the DC's unerring ability to regain office after an exit. Yet, three similarities across Italian governing parties are noteworthy. First, they tended to overturn cabinets only when assigned long-term governing roles in a coalition formula, as detailed in Subtable 5B. This pattern applied even to the leftmost PSI, excluded from the centrist formula, and the rightmost PLI, excluded from center-left coalitions. As argued, the security and stability of coalition formulae freed component parties to destabilize cabinets. Second, parties responsible for cabinet collapses rather often ruled in successor governments, especially after 1976, when the PSI was pivotal. Finally, parties responsible for breakups more often than not triggered a change in government composition. By this criterion, the policy con-

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[19] These measures have shortcomings. For instance, two DC-only cabinets may have distinct policy colorations, and not all governing parties influence policy to the same extent. Yet, parties in government clearly have the potential to shape policy, as is captured by the measures (K. Strom 1990b, 42–4). In ongoing research, I am probing alternative indices of the policy costs and benefits of governing, measures that are based on content analyses of parties' election manifestos and of authoritative policy documents issued by governments (Budge, Robertson, and Hearl 1987; Laver and Budge 1992).

**TABLE 5. Party Status in Government and Coalition Formula, and Government Composition, at and after Government Collapses, by Party Responsible for Government Collapse, 1946–92**

| | Party Responsible for Collapse at Time $t - 1$ | | | | |
|---|---|---|---|---|---|
| | DC | PSI | PSDI | PRI | PLI |
| | | | All Collapses 1946–92 | | |
| A. Status in Government at $t - 1$ | | | | | |
| In government | 100.0% | 62.5% | 66.7% | 25.0% | 25.0% |
| Out of government | 0 | 37.5 | 33.3 | 75.0 | 75.0 |
| Total | 100.0% | 100.0% | 100.0% | 100.0% | 100.0% |
| B. Status in Coalition Formula at $t - 1$ | | | | | |
| In coalition formula | 100.0% | 84.4% | 100.0% | 87.5% | 87.5% |
| Out of coalition formula | 0 | 15.6 | 0 | 12.5 | 12.5 |
| Total | 100.0% | 100.0% | 100.0% | 100.0% | 100.0% |
| C. Status in Government at $t$ | | | | | |
| In government | 100.0% | 59.4% | 50.0% | 50.0% | 50.0% |
| Out of government | 0 | 40.6 | 50.0 | 50.0 | 50.0 |
| Total | 100.0% | 100.0% | 100.0% | 100.0% | 100.0% |
| D. Government Composition at $t$ | | | | | |
| Parties change after fall | 58.1% | 68.8% | 72.2% | 56.3% | 50.0% |
| Parties same as before fall | 41.9 | 31.2 | 27.8 | 43.8 | 50.0 |
| Total | 100.0% | 100.0% | 100.0% | 100.0% | 100.0% |
| N collapses | (31) | (32) | (18) | (16) | (8) |
| | | | Coalition Collapses 1946–92 | | |
| E. Status in Government at $t$ | | | | | |
| In government | 100.0% | 68.2% | 45.5% | 55.6% | 66.7% |
| Out of government | 0 | 31.8 | 54.5 | 43.8 | 33.3 |
| Total | 100.0% | 100.0% | 100.0% | 100.0% | 100.0% |
| F. Government Composition at $t$ | | | | | |
| Parties change after fall | 50.0% | 68.2% | 72.7% | 66.7% | 66.7% |
| Parties same as before fall | 50.0 | 31.8 | 27.3 | 33.3 | 33.3 |
| Total | 100.0% | 100.0% | 100.0% | 100.0% | 100.0% |
| N coalition collapses | (18) | (22) | (11) | (9) | (3) |
| | | | All Collapses 1976–92 | | |
| G. Status in Government at $t$ | | | | | |
| In government | 100.0% | 75.0% | 100.0% | 80.0% | 100.0% |
| Out of government | 0 | 25.0 | 0 | 20.0 | 0 |
| Total | 100.0% | 100.0% | 100.0% | 100.0% | 100.0% |
| H. Government Composition at $t$ | | | | | |
| Parties change after fall | 54.5% | 62.5% | 50.0% | 60.0% | 66.7% |
| Parties same as before fall | 45.5 | 37.5 | 50.0 | 40.0 | 33.3 |
| Total | 100.0% | 100.0% | 100.0% | 100.0% | 100.0% |
| N collapses | (11) | (16) | (2) | (5) | (3) |

*Source*: Calculations based on coding of information in *Keesing's Contemporary Archives*; and government status data in Appendix A.

*Note*: Entries are the percentage of cases in which a party (or government) had the designated status. A "coalition formula" refers to a relatively durable coalition design (e.g., the centrist formula, composed of the DC, PSDI, PRI, and PLI, lasted from 1947 to 1963); a party included in a formula need not serve in every cabinet ruling under the formula (e.g., the PLI served in five of the 17 cabinets formed under centrism, as Appendix A details). Parties are coded as included in coalition formulae during the following periods: DC—1946–92; PSI—1946–47, 1963–76, 1979–92; PSDI—1947–76, 1979–92; PRI—1947–76, 1979–92; PLI—1947–63, 1979–92.

sequences of rupturing coalitions or one-party cabinets, before or after the PSI became pivotal, are roughly alike.

From 1976 to 1992, the PSI decided governments' fate with astonishing frequency. At the same time, the contest between the DC and the pivotal PSI altered the ties between the executive and the legislature. Cabinets met increased difficulties in relying on parliamentary majorities. As one sign of those difficulties, the first Italian government with a Socialist premier—Craxi I, a surplus coalition—saw defectors reduce its support to a minority 163 times (Di Scala 1988, 224). As a result of such challenges, executive decrees became much more common after 1976 (Della Sala 1987, 39; Nocifero and Valdini 1992, 19). These changes generated reactions in turn. Legislation in August 1988 limited the conditions under which executive decrees could be issued, and in October 1988 the Chamber of Deputies radically circumscribed use of the secret ballot (Barrera 1989). Further institutional reforms are discussed below.

***Bargaining Costs.*** As indexed by time, the bargaining costs of launching Italian governments are high. On average, Italian politicians spend a month in negotiating over new governments, substantially longer than politicians in most other parliamentary democracies (1946–87 data for fifteen nations in K. Strom 1990b, 144, 67). Despite the invention of devices to simplify bargaining, government crises in Italy have become more protracted during the postwar period. In particular, crises averaged 23 days between the 1946 and 1976 elections, as opposed to 52 days between 1976 and 1992, when the DC faced a pivotal PSI (calculated from data in Pollio Salimbeni 1989 and *Keesing's Contemporary Archives*).[20]

Yet, time invested in bargaining should also be evaluated for what it returns. From 1946 to 1992 in Italy, as shown, the office costs of building cabinets, even surplus coalitions, were low. Some parties under some conditions achieved office benefits from undoing governments. Breakups tended to bring influence over policy. Most voters appeared to be "indulgent [and] . . . rapidly forgetful" about cabinet collapses (a former deputy quoted in *Corriere della Sera*, June 28, 1986).

In such a system, as Andreotti summarized in a noted saying, "power wears out those without it," and power reinforces the powerful. To judge from events since the 1992 elections, that Italy no longer exists. The roots of the transformation lie in the two classes of explanatory factors highlighted here. I now take up evidence bearing out this claim.

***Clinching Evidence.*** The April 1992 parliamentary elections marked a profound shift in spatial and institutional conditions in Italy. The Lombard/Northern League and the Rete (anti-Mafia Network) recast dimensions of party competition and campaigned against corruption. The DC's support dipped below 30%, which meant that the DC no longer qualified as a core party (Schofield 1993). The 1992 elections were the first national elections held after the secret ballot was restricted in Parliament, the single preference vote introduced, and the PCI transformed into the Democratic Party of the Left [Partito Democratico della Sinistra (PDS)]. According to my argument, some change in the costs of coalition and cost-reduction strategies should have ensued.

The first postelection coalition joined the same four parties that governed on election eve. But the allies were led by Italy's second Socialist premier, their coalition had minimal winning size, and they divided among them a total of sixty portfolios. The preelection coalition contained 99 portfolios overall. The second postelection government was headed by Italy's first nonparty premier, Carlo Azeglio Ciampi, who almost succeeded in allying with the PDS, Greens, and PRI; last-minute disagreements left him with a four-party minimal winning coalition, a new edition of its predecessor that allocated an unprecedented share of portfolios (12 out of 62) to nonparty experts.[21] In August 1993, pressured by referendum results and Ciampi's exhortations, Parliament passed new electoral laws, which combine plurality and proportional rules for Chamber of Deputies and Senate elections and impose a threshold of 4% for representation in the chamber.

Held under the new laws, the March 1994 elections produced even more sweeping change: the end of fifty years of uninterrupted DC incumbency. Public outrage at widespread corruption inflicted devastating losses on the Popular Party (as the DC renamed itself in January 1994) and the PSI. Undone by corruption, the PLI and PSDI did not even contest the elections. In May, media magnate Silvio Berlusconi became premier. His government embraced Forza Italia, the movement he founded in early 1994; the Northern League; the National Alliance, the renamed MSI; and the Centrist Union [Unione di Centro (UDC)] and the Christian Democratic Center [Centro Cristiano Democratico (CCD), a right-wing splinter from the DC], which were Forza Italia's electoral allies in some constituencies. The coalition contained 64 portfolios and by one standard had minimal winning size.[22] Berlusconi resigned in December 1994 when the Northern League withdrew. In January 1995 economist Lamberto Dini became Italy's second nonparty premier, guiding a cabinet made up of 55 ministers and undersecretaries. For the first time in postwar Italy, not a single member of Parliament was included in the executive. The Dini government passed its votes of investiture thanks to support from the PDS.

## CONCLUSION

In postwar Italy until 1992, transitory cabinets were staffed by permanent incumbents. These empirical outcomes make Italy an extreme but not unique case among parliamentary democracies. The outcomes pose vexing questions when viewed in the light of game-theoretic predictions. What explains the combination of instability and stability? How can governments break up at such low cost? The framework I develop accommodates this puzzle. I organize comparisons within Italy (across parties, across sets of governments, and across time) into numerous observable implications of my hypotheses. The evidence indicates that particular spatial and institutional conditions in Italy curbed the costs of assembling and dismantling coalitions and encouraged strategies that lowered costs further. When policy space and institutions were redefined in the early

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[20] Of course, politicians do not devote every minute to bargaining during the interregnum between governments. Nonetheless, it is reasonable to presume that a prime minister who enters office ten weeks after the resignation of a predecessor has expended more energy in negotiations than has a prime minister whose installation occurs ten days after the fall of the preceding government. (The former scenario characterizes the average Dutch government since the end of World War II; the latter, the average Swedish government [Mershon n.d.].) Coalition analysts and country specialists routinely point to long interregna as signs of bargaining complexity (e.g., King et al. 1990; Laver and Schofield 1990; on the Netherlands, Gladdish 1991; on Belgium, Timmermans 1994).

[21] Even though a PRI senator was undersecretary to the premier, the PRI did not consider itself a member of the coalition and abstained on the cabinet's investiture (*Corriere della Sera*, April 29–May 13, 1993).

[22] The Berlusconi government qualifies as minimal winning if electoral alliances (the Freedom Alliance and the Good Government Alliance) are counted as its component units. If parliamentary parties are counted as the units comprising the coalition, it has surplus status and a superfluous member in the CCD.

1990s, costs and outcomes in Italian coalition politics were transformed.

The analysis here is based on trade-offs. By focusing on one country, I am able to trace processes and tap diverse data sources in ways not possible in cross-national research. I trade breadth for depth in order to grapple with an anomaly that has remained unresolved to date. I ease the trade-off somewhat by referring to other scholars' cross-national findings (such as the electoral losses posted by governing parties in K. Strom 1990b). Future work could do more to explore the posited linkages among spatial and institutional conditions, costs incurred, and records of duration and turnover. Extensions of the research could take several forms.

The argument advanced here would be bolstered by cross-national evidence. A preliminary study (Mershon 1995) compares five countries that present a range of electoral and parliamentary institutions and also differ in configurations of parties and voters in policy space. The findings suggest that, not only for Italy but also for Finland, Ireland, the Netherlands, and Norway, spatial and institutional environments shape the costs of coalition that parties face and structure parties' efforts to deflate those costs. For instance, the country that most nearly resembles Italy in spatial terms, the Netherlands, displays relatively long-lived governments and fairly ample turnover. Dutch institutions, working through their influence on the costs of coalition, account for the difference in outcomes.

To appreciate the influence of Dutch institutions, consider the office costs of building coalitions. As Dutch coalitions have stretched from two to five parties, governmental offices overall have tended to rise, and the number of Christian Democratic (KVP/CDA) ministers and junior ministers has declined only slightly.[23] Yet, Dutch portfolio inflation is a faint echo of the Italian phenomenon: Dutch five-party coalitions average fewer than 15 junior ministers, whereas the relevant figure for Italy is more than 50. Why? Since Dutch governments do not confront votes of investiture, and since secret ballots are the exception rather than the rule in the Dutch Parliament, the builders of Dutch cabinets lack the incentives of their Italian counterparts to use office as "glue" to bind assertive parliamentarians to governments. Disincentives operate as well. The addition of cabinet posts can trigger resignations in Dutch parliamentary parties, for ministers and junior ministers in the Netherlands cannot hold parliamentary seats (Andeweg and Bakema 1994, 66; Gladdish 1991, 59). Finally, unlike the Italian DC, the KVP/CDA lost its core status a few times—while retaining office—before the early 1990s. In ongoing research I am expanding the analysis to encompass a larger set of countries.

A second category of extensions involves increasing the observable implications of my hypotheses for Italy and each other country examined (cf. King, Keohane, and Verba 1994). The logical question is: If the reasoning is correct, what else may be affected by spatial and institutional conditions (besides the costs of coalition as measured here)? Among other things, under institutions roughly like those in pre-1992 Italy, bargaining among a core party's factions should be altered by the appearance of a pivotal party. Under national-level spatial and institutional conditions approximating those in pre-1992 Italy, individual undersecretaries who reap electoral benefits from governing (e.g., amass many preference votes) should rise to ministerial positions. Data collection at the different levels of analysis is a daunting task, but some evidence in favor of these hypotheses may be gleaned from existing studies. Venditti (1981, 150–9) documents that the pivotal PSI's decision to reenter government after the 1979 elections prompted a realignment among DC factions, the elimination of DC politicians lacking a clear factional affiliation from the competition for cabinet office, and a more rigid and exact application of the Cencelli manual. Tracking the careers of Italian politicians from 1946 to 1976, Calise and Mannheimer (1982, 99–101) show that undersecretaries who attracted relatively many preference votes once they entered government tended to be promoted to the ministerial ranks.

Another way to acquire more observable implications would be to consider corruption. If spatial and institutional conditions influence parties' ability to modify or maneuver around the costs of coalition, what does that mean for parties' ability to extract illegal benefits from governing? In particular, are parties with patronage-oriented voters especially prone to corruption? Are cost-management strategies applied to corruption management? Although gathering data on bribes and kickbacks is arduous, evidence now available for Italy answers the last two questions: Yes. Ricolfi (1993) finds that in the 1987–92 Parliament the PSDI was the party most prone to corruption. Della Porta (1993) documents that in Milan corrupt politicians used funds from bribes to campaign for preference votes and establish a personal following of voters willing to trade votes for favors. Plans for proportional division of spoils extended to the sharing of kickbacks among parties.

Additional items on the research agenda could be named, but it is much more important to stress the bottom line. Numerous within-nation comparisons indicate that, given the spatial and institutional conditions in postwar Italy until 1992, parties paid low prices in coalition politics and easily set prices even lower. Since this explanation accommodates what is otherwise an anomaly, it advances the general understanding of coalition bargaining. Preliminary evidence on additional observations beyond the main body of the Italian data support the inference that this framework comprehends Italy's extremes and the degrees of stability registered in other parliamentary democracies.

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[23] The Catholic People's Party [Katholieke Volkspartij (KVP)] or its heir, the Christian Democratic Appeal [Christen-Democratisch Appèl (CDA), which joined the KVP and two Protestant parties in 1977], has occupied the core of the Dutch policy space for most of the postwar period.

## APPENDIX A

### TABLE A-1. Italian Governments, 1946–96

| Government | Date Formed | Date Resigned | Parties in Cabinet | Seats in Chamber (%) | Type |
|---|---|---|---|---|---|
| De Gasperi II | 7/46 | 1/47 | DC + PCI + PSI[a] + PRI | 81 | S |
| De Gasperi III | 2/47 | 5/47 | DC + PCI + PSI | 67 | S |
| De Gasperi IV | 5/47 | 12/47 | DC | 37 | MIN |
| De Gasperi V | 12/47 | 5/48 | DC + PSDI[a] + PRI + PLI | 58 | S |
| De Gasperi VI | 5/48 | 10/49 | DC + PSDI + PRI + PLI | 64 | S |
| De Gasperi VII | 11/49 | 1/50 | DC + PRI + PLI | 58 | S |
| De Gasperi VIII | 1/50 | 4/51 | DC + PSDI + PRI | 60 | S |
| De Gasperi IX | 4/51 | 7/51 | DC + PRI | 55 | S |
| De Gasperi X | 7/51 | 6/53 | DC + PRI | 55 | S |
| De Gasperi XI | 7/53 | 7/53 | DC | 45 | MIN |
| Pella | 8/53 | 1/54 | DC | 45 | MIN |
| Fanfani I | 1/54 | 1/54 | DC | 45 | MIN |
| Scelba | 2/54 | 6/55 | DC + PSDI + PLI | 50 | MW[b] |
| Segni I | 7/55 | 5/57 | DC + PSDI + PLI | 50 | MW[b] |
| Zoli | 5/57 | 6/58 | DC | 45 | MIN |
| Fanfani II | 7/58 | 1/59 | DC + PSDI | 49 | MIN |
| Segni II | 2/59 | 2/60 | DC | 46 | MIN |
| Tambroni | 3/60 | 7/60 | DC | 46 | MIN |
| Fanfani III | 7/60 | 2/62 | DC | 46 | MIN |
| Fanfani IV | 2/62 | 5/63 | DC + PSDI + PRI | 51 | MW |
| Leone I | 6/63 | 11/63 | DC | 41 | MIN |
| Moro I | 12/63 | 6/64 | DC + PSI + PSDI + PRI | 61 | S |
| Moro II | 7/64 | 1/66 | DC + PSI + PSDI + PRI | 61 | S |
| Moro III | 2/66 | 6/68 | DC + PSI + PSDI + PRI | 61 | S |
| Leone II | 6/68 | 11/68 | DC | 42 | MIN |
| Rumor I | 12/68 | 7/69 | DC + PSI-PSDI + PRI | 58 | S |
| Rumor II | 8/69 | 2/70 | DC | 42 | MIN |
| Rumor III | 3/70 | 7/70 | DC + PSI + PSDI + PRI | 58 | S |
| Colombo I | 8/70 | 3/71 | DC + PSI + PSDI + PRI | 58 | S |
| Colombo II | 3/71 | 1/72 | DC + PSI + PSDI | 57 | S |
| Andreotti I | 2/72 | 2/72 | DC | 42 | MIN |
| Andreotti II | 6/72 | 6/73 | DC + PSDI + PLI | 50 | MW[b] |
| Rumor IV | 7/73 | 3/74 | DC + PSI + PSDI + PRI | 59 | S |
| Rumor V | 3/74 | 10/74 | DC + PSI + PSDI | 57 | S |
| Moro IV | 11/74 | 1/76 | DC + PRI | 45 | MIN |
| Moro V | 2/76 | 4/76 | DC | 42 | MIN |
| Andreotti III | 7/76 | 1/78 | DC | 42 | MIN |
| Andreotti IV | 3/78 | 1/79 | DC | 42 | MIN |
| Andreotti V | 3/79 | 3/79 | DC + PSDI + PRI | 46 | MIN |
| Cossiga I | 8/79 | 3/80 | DC + PSDI + PLI | 46 | MIN |
| Cossiga II | 4/80 | 9/80 | DC + PSI + PRI | 54 | S |
| Forlani | 10/80 | 5/81 | DC + PSI + PSDI + PRI | 57 | S |
| Spadolini I | 6/81 | 8/82 | PRI + DC + PSI + PSDI + PLI | 58 | S |
| Spadolini II | 8/82 | 11/82 | PRI + DC + PSI + PSDI + PLI | 58 | S |
| Fanfani V | 12/82 | 4/83 | DC + PSI + PSDI + PLI | 56 | S |
| Craxi I | 8/83 | 6/86 | PSI + DC + PSDI + PRI + PLI | 58 | S |
| Craxi II | 8/86 | 3/87 | PSI + DC + PSDI + PRI + PLI | 58 | S |
| Fanfani VI | 4/87 | 4/87 | DC | 37 | MIN |
| Goria | 7/87 | 3/88 | DC + PSI + PSDI + PRI + PLI | 60 | S |
| De Mita | 4/88 | 5/89 | DC + PSI + PSDI + PRI + PLI | 60 | S |
| Andreotti VI | 7/89 | 3/91 | DC + PSI + PSDI + PRI + PLI | 60 | S |
| Andreotti VII | 4/91 | 4/92 | DC + PSI + PSDI + PLI | 56 | S |
| Amato | 6/92 | 4/93 | PSI + DC + PSDI + PLI | 53 | MW |
| Ciampi | 4/93 | 1/94 | DC + PSI + PDSI + PLI | 53 | MW |
| Berlusconi | 5/94 | 12/94 | FI [+ CCD[c]] + League + AN | 58 | MW[c] |
| Dini | 1/95 | 1/96 | nonpartisan | | |

*Sources*: Farneti 1983, 36–9; Petracca 1980; Pollio Salimbeni 1989; Strom 1990b, 140–1; *Corriere della Sera*, various issues.

[a] This table always designates the Socialists as the PSI, even though the party had a different name before 1947 (when the Social Democrats exited) and from 1966 to 1969 (when the Socialists and Social Democrats reunited). Similarly, the table always refers to the Social Democrats as the PSDI.

[b] Exactly half of seats controlled by coalition.

[c] The Christian Democratic Center (CCD) allied with Forza Italia on the PR ballot in the 1994 elections but then constituted a separate parliamentary group. If parliamentary parties are counted as the units comprising the Berlusconi coalition, it has surplus status.

*Key to cabinet types*:
S = surplus coalition (total = 28); MW = minimal winning coalition (total = 7); MIN = minority government (total = 20)

## APPENDIX B

**TABLE B-1. Italian Electoral Results, 1946–92: Votes and Seats Won in Elections to the Chamber of Deputies**

| | Year of Election | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Party | 1946 | 1948 | 1953 | 1958 | 1963 | 1968 | 1972 | 1976 | 1979 | 1983 | 1987 | 1992 |
| | Votes (percentage share of valid votes) | | | | | | | | | | | |
| Greens | — | — | — | — | — | — | — | — | — | — | 2.5 | 2.8 |
| DP | — | — | — | — | — | — | — | 1.5 | 0.8 | 1.5 | 1.7 | — |
| PDUP | — | — | — | — | — | — | 0.7 | w/DP | 1.4 | w/PCI | — | — |
| PSIUP | — | — | — | — | — | 4.4 | 1.9 | — | — | — | — | — |
| PR | — | — | — | — | — | — | — | 1.1 | 3.5 | 2.2 | 2.6 | — |
| RC | — | — | — | — | — | — | — | — | — | — | — | 5.6 |
| PCI[a] | 18.9 | 31.0[b] | 22.6 | 22.7 | 25.3 | 26.9 | 27.2 | 34.4 | 30.4 | 29.9 | 26.6 | 16.1 |
| PSI | 20.7 | | 12.7 | 14.2 | 13.8 | 14.5[c] | 9.6 | 9.6 | 9.8 | 11.4 | 14.3 | 13.6 |
| PSDI | — | 7.1 | 4.5 | 4.6 | 6.1 | | 5.1 | 3.4 | 3.8 | 4.1 | 2.9 | 2.7 |
| PRI | 4.4 | 2.5 | 1.6 | 1.4 | 1.4 | 2.0 | 2.9 | 3.1 | 3.0 | 5.1 | 3.7 | 4.4 |
| DC | 35.1 | 48.5 | 40.1 | 42.3 | 38.3 | 39.1 | 38.7 | 38.7 | 38.3 | 32.9 | 34.3 | 29.7 |
| PLI | 6.8 | 3.8 | 3.0 | 3.5 | 7.0 | 5.8 | 3.9 | 1.3 | 1.9 | 2.9 | 2.1 | 2.8 |
| League | — | — | — | — | — | — | — | — | — | — | 1.3 | 8.7 |
| PM-PNM | 2.8 | 2.8 | 6.9 | 4.8 | 1.7 | 1.3 | 8.7[d] | 6.1[d] | — | — | — | — |
| MSI | — | 2.0 | 5.8 | 4.8 | 5.1 | 4.5 | | | 5.3 | 6.8 | 5.9 | 5.4 |
| Other | 11.3 | 2.3 | 2.8 | 1.7 | 1.3 | 1.5 | 1.3 | 0.8 | 1.8 | 3.2 | 2.1 | 8.2 |
| | Seats (number) | | | | | | | | | | | |
| RC | — | — | — | — | — | — | — | — | — | — | — | 35 |
| PCI[a] | 104 | 183[b] | 143 | 140 | 166 | 177 | 179 | 227 | 201 | 198 | 177 | 107 |
| PSI | 115 | | 75 | 84 | 87 | 91[c] | 61 | 57 | 62 | 73 | 94 | 92 |
| PSDI | — | 33 | 19 | 22 | 33 | | 29 | 15 | 21 | 23 | 17 | 16 |
| PRI | 23 | 9 | 5 | 6 | 6 | 9 | 15 | 14 | 15 | 29 | 21 | 27 |
| DC | 207 | 305 | 263 | 273 | 260 | 266 | 266 | 263 | 261 | 225 | 234 | 206 |
| PLI | 41 | 19 | 13 | 17 | 39 | 31 | 20 | 5 | 9 | 16 | 11 | 17 |
| League | — | — | — | — | — | — | — | — | — | — | 1 | 55 |
| MSI | — | 6 | 29 | 24 | 27 | 24 | 56[d] | 35[d] | 31 | 42 | 35 | 34 |
| Other | 66 | 19 | 43 | 30 | 12 | 32 | 4 | 14 | 30 | 24 | 40 | 41 |
| Total | 556 | 574 | 590 | 596 | 630 | 630 | 630 | 630 | 630 | 630 | 630 | 630 |

*Source*: Farneti 1983, 36–9; Rhodes 1988, 187; Sani 1994, 41.

*Note*: Intercolumnar brackets refer to joint slates. Dashes indicate that the party did not contest the election in question.

[a] Democratic Party of the Left in 1992.

[b] Joint slate of Communists and Socialists.

[c] Reunified Socialists and Social Democrats.

[d] Joint slate of Monarchists and Italian Social Movement.

*Key to party acronyms*:

DP = Proletarian Democracy; PDUP = Party of Proletarian Unity; PSIUP = Italian Socialist Party of Proletarian Unity; PR = Radical Party; RC = Communist Refoundation; PCI = Italian Communist Party; PSI = Italian Socialist Party; PSDI = Italian Social Democratic Party; PRI = Italian Republican Party; DC = Christian Democracy; PLI = Italian Liberal Party; PM-PNM = Monarchist Party; MSI = Italian Social Movement.

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# Dynamics of Cosponsorship

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*Electoral-connection theories of legislative politics view bill cosponsorship as low-cost position taking by rational legislators who communicate with target audiences (e.g., constituents) external to the legislature. Legislative signaling games suggest a view of bill cosponsorship in which early cosponsors attempt to communicate to target audiences (e.g., the median voter) within the legislature. Using data from the 103rd U.S. House of Representatives, we show that the timing of legislators' cosponsorship decisions are more supportive of cosponsorship as intralegislative signaling than as extralegislative position taking. First, policy extremists on both sides of the political spectrum are more likely than moderates to be initial endorsers of legislative initiatives. Second, extremist-moderate differences diminish over the course of bill histories.*

Few works have been more influential on legislative studies in recent decades than Mayhew's *Congress: The Electoral Connection*. A key element of electoral-connection theories is position taking: legislators' public endorsements of legislative proposals without regard to the usually grim prospects that such proposals will pass. A stereotypical example of such behavior is bill cosponsorship. Since "in a large class of legislative undertakings, the electoral payoff is for *positions* not *effects*" (Mayhew 1974, 132), a legislator seeking reelection could not hope for a more efficient way to strengthen his or her electoral connection than to cosponsor constituency-prized legislative initiatives. Legislators who cater to their theoretical principals—politically relevant constituents outside the legislature—in this manner reap large benefits at small cost. Contrast this cost-benefits calculus with that for intralegislative work, for which costs of passing bills are large and electoral benefits are small, and the case for interpreting cosponsorship as an instance of electoral-connection theory at work becomes all the more compelling.

A body of more recent theoretical research has portrayed legislators in a somewhat different but not inherently contradictory light. Signaling theories of legislative politics postulate that legislators are exclusively outcome oriented and thus care about public pronouncements about policies only insofar as these are means to law-making ends. For example, in a model of committee-floor decision making, Gilligan and Krehbiel (1989) identify conditions under which "heterogeneous signalers" (policy advocates from both sides of a policy spectrum relative to the median voter) can credibly transmit private information about the merits of a legislative proposal to uninformed moderates within the legislature. The outcome associated with the resulting policy choice corresponds closely with the chamber median voter's ideal point. It is also utility-enhancing for the heterogeneous signalers, because uncertainty about the consequences of policy is reduced. As in Mayhew's theory, legislators in signaling theories are conscious of, and sensitive to, the consequences of their positions and statements. In signaling theories, however, the principal audience for such statements is internal rather than external to the legislature. Thus, Young and Wilson (1993, 21) fruitfully apply some insights from these theories to their study of bill cosponsorship, describing cosponsorship as a mechanism that "signals the degree of support [for legislation], signals an ideological base, and signals expertise."

Upon closer inspection, these two classes of theories may offer divergent predictions about patterns of cosponsorship. If so, then data on cosponsorship can provide a unique opportunity for systematic, comparative tests of the theories.

Electoral-connection theories predict a close correspondence between legislators' ideological predispositions (or, by extension, those of their reelection constituencies) and the content of the legislation they choose to cosponsor. In other words, liberals should cosponsor liberal bills, conservatives should cosponsor conservative bills, and moderates should cosponsor moderate bills. With an added and empirically plausible assumption that moderate bills are more likely to pass than extreme bills, the theory also suggests that moderate cosponsorship should be most prevalent among winning bills and, likewise, more prevalent than extremist cosponsorship on such bills. Finally, electoral connection theories suggest that, controlling for preference effects, legislators with uncertain electoral prospects (marginal seats) should engage in cosponsorship activity at greater rates than their electorally secure colleagues.

Signaling theories of legislatures have quite different implications for bill cosponsorship. If the primary audience for acts of cosponsorship is internal rather than external—more specifically, if the intended receiver of early cosponsorship signals is the legislature's pivotal voter—then incomplete information theories suggest that confirmatory signaling by heterogeneous cosponsors is a viable way of building support for proposals that are in the interests of large majorities. A common pattern, therefore, should be early cosponsorship by relative extremists from both sides of the preference spectrum. Thereafter, bills should receive more broad-based endorsements by extremists and moderates alike, who, having received the prior credible signals, positively update their beliefs about the desirability of the proposed policies.

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Using data from the 103rd U.S. House of Representatives, this paper shows that the timing of cosponsorship decisions and the association between legislators' preferences and their cosponsorship decisions are more consistent with the predictions of intralegislative signaling theories than with extralegislative position-taking theories. The study is organized into the following sections: a review of previous research on cosponsorship, a framework for measuring and interpreting legislative dynamics, a description of the data, a presentation of an empirical model, a presentation of the empirical results, a discussion and further exploration of various interpretations of the results, and a summary that identifies directions for future research on legislative dynamics.

## COSPONSORSHIP

Although the right of members of the House of Representatives to cosponsor legislation was expressly denied in 1909 reform efforts, the House reconsidered and officially endorsed cosponsorship in 1967. In 1978 another rules change removed the House's prior limit of 25 cosponsors per bill. Throughout the 1980s and into the 1990s, cosponsorship has been common in the House and Senate alike.[1]

Previous research on bill cosponsorship provides a broad base on which to build. For example, Campbell (1982) studied cosponsorship in the House and Senate and found a positive relationship between liberalism and cosponsorship and a negative relationship between electoral margin and cosponsorship. Schiller (1995) studied bill sponsorship in the Senate and found positive relationships between primary sponsorship and liberalism, staff size, and various institutional position variables. Other researchers have focused on specific pieces of legislation. Regens (1989), for instance, found strong preference and constituency-characteristic effects on cosponsorship for environmental legislation, while Krehbiel (1995) showed that cosponsorship of and waffling on the House's A to Z spending plan was related to budgetary preferences. Cosponsorship also has been considered in the context of policy expertise. Browne and Ringquist (1985) questioned the relationship between cosponsorship and specialization in a study of state legislatures' decision making regarding the aged. Gilligan and Krehbiel (1995), however, offered evidence supportive of a positive relationship between cosponsorship and legislative expertise within the House Energy and Commerce Committee. Finally, several studies have estimated the relationship between the number of cosponsors on a bill and its prospects for clearing various hurdles in the legislative process, up to and including final passage. Browne (1985) found a positive relationship between cosponsorship and passage of bills in four U.S. states and suggested that cosponsorship may be a "bandwagon" phenomenon. Young and Wilson (1993), likewise, considered bandwagon, ideological, and expertise effects on legislative success and reported mixed findings for the U.S. House of Representatives depending on the legislative stage.

While these findings demonstrate convincingly that cosponsorship has systematic components, they are somewhat limited in terms of two primary concerns here: comparative testing of the hypotheses of different theories and econometric attentiveness to the dynamic patterns of cosponsorship. To be sure, the studies without exception implicitly portray the legislative process as one in which dynamics play a fundamental role. Coalition building takes place over time. Individual cosponsorship decisions precede the votes on and passage of legislation. Cues by cosponsors must be given before they are taken by subsequent cosponsors or voters. Specialization typically occurs prior to legislative signaling and legislative choice. Yet, although interpretations of cosponsorship findings are generally dynamic, the supporting data analysis has been static. That is, studies of cosponsorship typically analyze the causes or consequences of legislators' cosponsorship choices, but they are not econometrically sensitive to when legislators jump onto the legislative bandwagon for specific bills.[2] What is needed, then, is a general way of thinking about the dynamics of cosponsorship and a corresponding empirical method to test for the existence of hypothetical dynamic patterns suggested by different theories.

## MEASURING LEGISLATIVE DYNAMICS

Duration data measure events that occur over time, such as the life of an organism, the operation of a machine, the strike arising out of a labor dispute, or the formation of a legislative coalition. The variable of interest in the analysis of duration data is the elapsed time from the beginning of an event until its termination. In the case of cosponsorship, that variable is the elapsed time prior to a legislator's decision to cosponsor a bill.

Most legislators' cosponsorship decisions are never observed because, of course, not all bills are cosponsored by all legislators. Even among heavily cosponsored bills, the bills pass or sessions of Congress end before several legislators have made explicit decisions regarding cosponsorship. This is an example of a common property of duration data: censoring, or the cessation of measurement. For this reason, conventional regression analysis is poorly suited to the analysis of duration data in general and to the dynamics of legislative decision making in particular.

If the dependent variable is *Time until Cosponsorship*, how should noncosponsoring legislators be coded? Simply omitting them from a regression analysis would lead to biased estimates of the effects of the determinants of the cosponsorship decision. Thus, alternative data collection and estimation methods are used to analyze duration data. For each occurrence of an event, duration data include information on both the elapsed time of the event and whether the measurement of the event was censored. With this information, we can estimate the influence of the determinants of duration by specifying a

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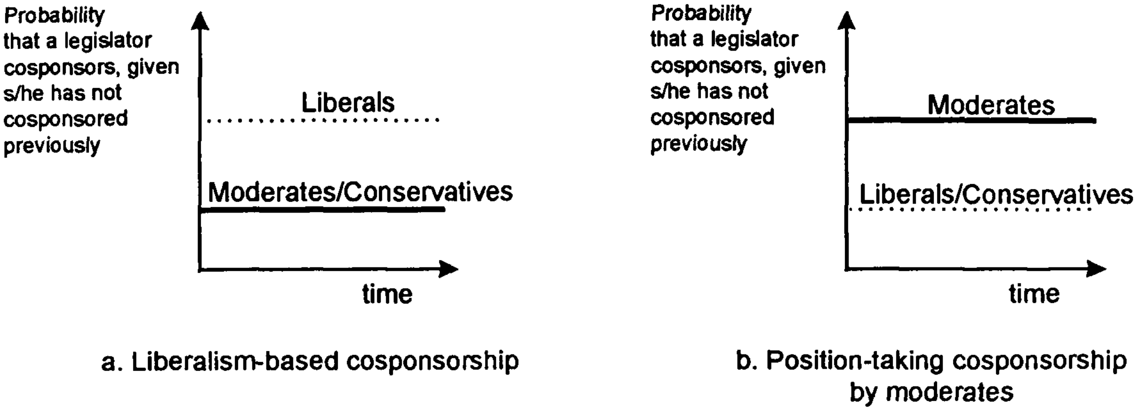
[1] See Young and Wilson 1993 for a more extensive historical review.

[2] Young and Wilson 1993 is a debatable exception. While they estimate equations on several different stage-dependent variables, the timing of individual cosponsorship decisions plays no role in their analysis.

**FIGURE 1. Static Effects of Preferences on Cosponsorship**



a. Liberalism-based cosponsorship

b. Position-taking cosponsorship by moderates

hazard function, which maps the determinants of duration and time elapsed in the occurrence of the event into the probability of the event occurring, conditional on it not having yet occurred. In the case of cosponsorship, the hazard function specifies the probability at a point in time of a legislator cosponsoring a bill, conditional on the legislator not having yet cosponsored.

The findings of previous studies of legislative dynamics are expressed in terms of hazard functions and differences between hazard functions, as shown in Figure 1. In Figure 1a, cosponsorship is a function of liberalism or preferences for big government. Thus, liberals' hazard rate is consistently (and constantly) greater than that for moderates and conservatives. In Figure 1b, cosponsorship is again preference based, but here moderates cosponsor at greater rates than liberals and conservatives. This would happen, for example, when legislation tends to be moderate and cosponsorship is motivated by position taking, as in electoral-connection theory. In line with previous literature, absolute hazard rates and differences between hazard rates are constant over time in Figure 1. This is the implicit assumption in any study that does not explicitly consider the timing of cosponsorship decisions. Inferences about dynamics of cosponsorship, therefore, are not strictly justifiable in such studies.

It is straightforward, however, to illustrate genuine dynamics within this basic diagrammatic scheme. Figures 2a–d show four types of preference-based dynamics. In this context, the terms *inside* and *outside* refer to relative locations on an ideological spectrum, not to strategies internal and external to the legislature.

Figure 2a depicts a pattern like Figure 1b in the early period. That is, first-on-board cosponsors are more likely to be extremists (liberals and conservatives combined) than moderates. As the process unfolds, however, this difference diminishes until moderates and extremists no longer have significantly different hazard rates. Thus, this dynamic pattern is called *outside-in cosponsorship* and is roughly consistent with a narrow theoretical view of cosponsorship as confirmatory signaling. It is also consistent with a broader empirical view of bipartisan coalition-building in which ideologically diverse entrepreneurs jointly mobilize for coalition-building in which, increasingly, moderates are brought on board.

Figure 2b illustrates a mirror image dynamic in which first-on-board cosponsors are somewhat more likely to be extremists than moderates but in which this difference grows over time. This pattern may seem somewhat implausible, but it is empirically discernible, nonetheless.

Figure 2c illustrates *inside-out cosponsorship*. First-on-board cosponsors are disproportionately moderates who, as the bill history transpires, seem increasingly to persuade extremists on both sides of the policy spectrum that the bill has merits and deserves their endorsement. This might reflect bipartisan coalition-building in which moderate party leaders play an entrepreneurial role and, over time, gain the support of their respective, relatively ideological backbenchers.

Figure 2d completes the illustrations. As in Figure 2c, cosponsorship begins as a somewhat more moderate than extremist phenomenon. In contrast to Figure 2c, however, this difference becomes more pronounced over time. A substantive interpretation of this pattern of cosponsorship may be that the give and take associated with coalition-building serve to placate moderates but make extremists disillusioned.

Although the patterns of dynamics in Figure 2 are not exhaustive,[3] they demonstrate that the effect of legislators' preferences on the timing of cosponsorship can distinguish among theories of legislative dynamics. To this end, we investigate two related questions. First, holding other legislator characteristics constant, do legislators with extreme liberal or conservative preferences cosponsor more or less frequently than do their more moderate counterparts? Second, if legislators with extreme preferences exhibit different cosponsorship behavior, is this difference manifested early or late in a bill's life?

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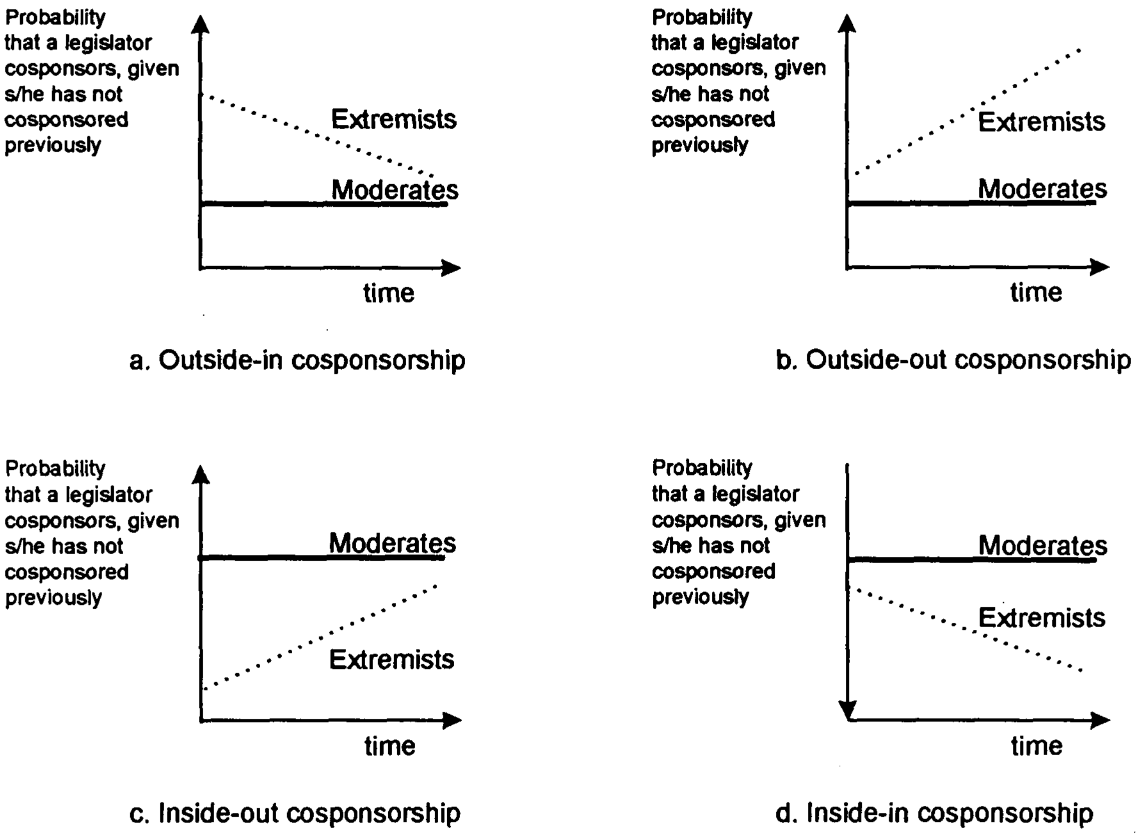
[3] For example, more explicitly partisan versions of these figures could be considered, as could variations of the patterns in which the lines crossed or bent.

FIGURE 2. Dynamic Effects of Preferences on Cosponsorship


Probability that a legislator cosponsors, given s/he has not cosponsored previously
Extremists
Moderates
time
Probability that a legislator cosponsors, given s/he has not cosponsored previously
Extremists
Moderates
time
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Moderates
Extremists
time
Probability that a legislator cosponsors, given s/he has not cosponsored previously
Moderates
Extremists
time

a. Outside-in cosponsorship
b. Outside-out cosponsorship
c. Inside-out cosponsorship
d. Inside-in cosponsorship

## THE DATA

To investigate the dynamics of cosponsorship, we used data on the 51 bills in the 103rd Congress with at least 50 cosponsors.[4] This yielded 51 bills × 435 legislators = 22,185 observations on opportunities for cosponsorship. Each bill's lifespan is partitioned into periods—early, intermediate, and late—during which legislators may choose to cosponsor the bill. The dependent variable in the analysis is then determined by a legislator's cosponsorship type for the given bill. For each legislator-bill observation, the legislator either never cosponsored the bill (0) or cosponsored it in the early (1), intermediate (2), or late (3) stage.

Bills, of course, have different lifespans, defined as the number of days elapsed between the bill's introduction and either its passage or the end of the 103rd Congress, whichever is sooner.[5] Unfortunately, this complicates the definition of the stages, since a model of the determination of the timing of the cosponsorship decision that applies across bills cannot define the stages of a bill's life (e.g., measure elapsed time-to-cosponsorship) in absolute terms, such as the number of days. If it did, then bills with short lives would appear to have more early-stage cosponsorship activity than bills with long lives, even if cosponsorship activity were uniformly distributed throughout each bill's life. To address this problem, the stages of a bill's life can be defined in terms relative to its own lifespan. The early stage of a bill's life lasts only one day, the day on which the bill is introduced.[6] The intermediate stage begins the day after a bill's introduction and lasts for 100 days or half the bill's life, whichever is shorter. The late stage lasts from the end of the intermediate stage until either the passage of the bill or the end of the Congress, whichever is sooner.

Tables 1 and 2 present descriptive statistics for the legislators ($N = 435$) and bills ($N = 51$), respectively. In Table 1, the first column of the first row shows that, on average, members of the 103rd Congress cosponsored 12.86 of the 51 highly cosponsored bills that we analyze. Some members did not cosponsor at all, and one member cosponsored 35 of the 51 bills. We report both means and various quantiles of the interest-group voting scores for comparability with previous work, although we use quantile-based measures of preference extremity in analysis, since some recent research suggests that voting scores provide only an ordinal measure of preferences (Snyder 1992, Groseclose 1992; see, however, Groseclose, Levitt, and Snyder 1995 and Londregan and Snyder 1994).

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[4] The selected nature of the sample means that inferences about the determination of the timing of cosponsorship do not necessarily apply to bills with a small number of cosponsors.

[5] In the 103rd Congress, bill lives ranged from 10 days (HR 4296, the ban on assault weapons) to 647 days (HR 25, reproductive rights).

[6] As a practical matter, this represents a longer but unmeasurable period during which the primary sponsor assembled first-on-board cosponsors through "Dear Colleague" letters, interstaff contacts, and so forth.

**TABLE 1. Descriptive Statistics for Legislator Characteristics ($N$ = 435)**

| | Mean | Standard Deviation | Minimum | Maximum |
|---|---|---|---|---|
| *N* bills cosponsored | 12.86 | 6.39 | 0 | 35 |
| Electoral margin | 30.01 | 23.03 | 1 | 100 |
| Years of tenure in house | 8.69 | 8.18 | 0 | 52 |
| ADA rating | 49.26 | 35.89 | 0 | 100 |
| 10th percentile: 4 | | | | |
| 25th percentile: 13 | | | | |
| 75th percentile: 85 | | | | |
| 90th percentile: 96 | | | | |
| ACU rating | 46.40 | 37.04 | 0 | 100 |

Table 2 reports important characteristics of each of the 51 bills in the sample, including lifespan, average number of days elapsed between introduction and cosponsorship among those who cosponsored, and the total number of cosponsors. In addition, the last three columns of Table 2 provide the Kaplan-Meier or raw hazard rate of cosponsorship for each stage of each bill's life, that is, the probability of cosponsorship, conditional on not yet having cosponsored. In other words, the Kaplan-Meier hazard rate for a given stage of a given bill's life is equal to the number of legislators who cosponsor at the given stage, divided by the number of legislators who have not cosponsored as of the end of the previous stage. The raw hazard rates show that a significant amount of cosponsorship activity occurs during the early stage, e.g., on the first day of a bill's life. For example, 96 legislators sponsored the Brady Bill (HR 1025) on the day it was introduced, leading to a hazard rate of 96/435 = 0.221. More generally, the last row of the table shows that, among the bills in the sample, the average hazard rate on the day of the bill's introduction was 0.079. (This is equal to the unconditional probability of cosponsorship, since no cosponsorship can occur before a bill's introduction.)

Cosponsorship activity continues through the intermediate and late stages of a bill's life (although this depends on the definition of the number of days included in each stage of a bill's life; indeed, cosponsorship per unit of calendar time declines precipitously after the day a bill is introduced). For some bills, both the conditional probability of cosponsorship and the total number of cosponsors are higher in the later stages of life. A total of 14 (0.032 × 435) legislators cosponsored HR 25, for example, on the day of its introduction; among the 421 legislators who had not cosponsored, 112 (0.266 × 421) did so during the intermediate stage of HR 25's life, and among the 309 who had not cosponsored by the end of the intermediate stage, 16 (0.052 × 309) cosponsored during the late stage of HR 25's life. Other bills exhibit the opposite pattern of cosponsorship. A total of 135 (0.310 × 435) members cosponsored HR 1308 on the day of its introduction, but only 21 (0.070 × 300) cosponsored in the intermediate stage, and only 13 (279 × 0.047) cosponsored in the late stage.

## AN EMPIRICAL MODEL

To identify the effect of legislator preferences on the timing of cosponsorship, we specify two empirical models of the cosponsorship hazard. The *constrained model* assumes that the influence of preference extremity is equal for liberals and conservatives. The *unconstrained model* allows the effect to vary depending on the legislator's position on the political spectrum. The models are specified according to the semiparametric proportional hazard form (see, for example, Han and Hausman 1990, Kessler 1996, and Meyer 1990). The model is semiparametric in that the effect of preferences on the hazard rate is allowed to be nonparametric, although the influence of nonpreference legislator characteristics takes a particular functional form that shifts the baseline hazard proportionally.

In the context of timing decisions on cosponsorship, estimating the baseline hazard as a nonparametric function of time and legislator preferences is a significant innovation over conventional proportional hazards models that either estimate the baseline according to some parametric functional form or do not estimate the baseline hazard at all (e.g., Box-Steffensmeier, Arnold, and Zorn 1995). The questions posed by theoretical frameworks, as discussed above, are twofold. Although the question regarding the influence of preferences on the *decision* to cosponsor or not to cosponsor can be answered with conventional proportional hazards models, the question regarding the influence of preferences on the *timing* of the decision to cosponsor cannot be answered with the conventional approach. Simply allowing preferences to shift the baseline hazard proportionally across time intervals does not address definitively the effect of preferences on timing, because the proportionality assumption may conflict with the predictions of timing generated by some of the theoretical models. If it does conflict, then hypothesis tests based on estimates from conventional models could not be used to distinguish among the theories. For example, a conventional proportional hazards model would not necessarily allow preference extremity to increase hazard rates by a greater amount early in a bill's life, if the early cosponsorship hazard for moderates were less than or equal to the late cosponsorship hazard for moderates.

We analyze legislators' cosponsorship decisions throughout three stages of each bill's life. An observation $i$ is a bill-legislator combination. For any such observation, cosponsorship either does not occur at all or else occurs during one of three stages of the bill's lifespan. Call these periods $t, t \in \{1, 2, 3\}$, where discrete period $t$ ends after $\tau(t, L_i)$ days have elapsed, and $L_i$ represents the lifespan of the bill in days.

A legislator-bill cosponsorship opportunity is also associated with a set of observable legislator characteristics $x_i$, such as tenure and electoral margin. In addition, preference extremity is measured by $p_i$ in the constrained model, where

$$p_i = \begin{cases} 1 \text{ if the legislator's ADA score is in the top} \\ \quad \text{or bottom quartile,} \\ 0 \text{ otherwise,} \end{cases}$$

**TABLE 2. Descriptive Statistics for Bill Characteristics**

| Bill Number | Description of Bill: (w)inner or (l)oser, (b)inding or (n)onbinding | Bill's Lifespan in Days | Average Days to Cosponsorship | Number of Cosponsors | Cosponsorship Hazard | | |
|---|---|---|---|---|---|---|---|
| | | | | | Early Stage | Intermediate Stage | Late Stage |
| HR 2 | Voter registration (w, b) | 30 | 14.76 | 71 | 0.064 | 0.000 | 0.106 |
| HR 8 | Child nutrition (w, b) | 560 | 229.32 | 62 | 0.005 | 0.000 | 0.139 |
| HR 25 | Reproductive rights (l, b) | 647 | 49.34 | 142 | 0.032 | 0.266 | 0.052 |
| HR 51 | DC statehood (l, b) | 366 | 0.00 | 77 | 0.177 | 0.000 | 0.000 |
| HR 324 | Crime against children (w, b) | 298 | 58.84 | 58 | 0.025 | 0.083 | 0.031 |
| HR 490 | DC hospitals (w, b) | 48 | 27.55 | 58 | 0.005 | 0.000 | 0.129 |
| HR 546 | Pension taxes (w, b) | 620 | 154.51 | 86 | 0.014 | 0.091 | 0.105 |
| HR 796 | Clinic access (w, b) | 288 | 85.72 | 127 | 0.002 | 0.187 | 0.128 |
| HR 877 | Smithsonian project (w, b) | 144 | 0.00 | 58 | 0.133 | 0.000 | 0.000 |
| HR 898 | USAF memorial (w, b) | 236 | 52.93 | 100 | 0.143 | 0.038 | 0.014 |
| HR 1012 | Organ donors (l, b) | 603 | 110.16 | 99 | 0.064 | 0.015 | 0.162 |
| HR 1025 | Brady bill/gun control (w, b) | 261 | 14.35 | 150 | 0.221 | 0.148 | 0.014 |
| HE 1133 | Crimes against women (w, b) | 269 | 75.22 | 221 | 0.136 | 0.258 | 0.233 |
| HR 1152 | Hate crimes (w, b) | 204 | 22.82 | 73 | 0.092 | 0.066 | 0.019 |
| HR 1161 | Crimes against elderly (l, b) | 592 | 28.91 | 57 | 0.087 | 0.035 | 0.013 |
| HR 1172 | Civil rights (l, b) | 591 | 43.03 | 114 | 0.154 | 0.095 | 0.036 |
| HR 1231 | Prevailing wages (l, b) | 589 | 22.59 | 56 | 0.002 | 0.053 | 0.078 |
| HR 1308 | Religious freedom (w, b) | 61 | 6.76 | 169 | 0.310 | 0.070 | 0.047 |
| HR 1527 | Food quality (l, b) | 561 | 120.37 | 223 | 0.090 | 0.154 | 0.367 |
| HR 1645 | Poverty data (w, b) | 207 | 45.86 | 58 | 0.060 | 0.056 | 0.023 |
| HR 1796 | Veterans' benefits (l, b) | 541 | 124.59 | 85 | 0.002 | 0.062 | 0.140 |
| HR 1900 | Worker privacy (l, b) | 534 | 111.61 | 157 | 0.030 | 0.220 | 0.155 |
| HR 1916 | Marine biotech (w, b) | 76 | 33.39 | 51 | 0.039 | 0.026 | 0.057 |
| HR 1933 | M. L. King holiday (w, b) | 320 | 53.78 | 100 | 0.110 | 0.083 | 0.056 |
| HR 2121 | Transportation rates (w, b) | 186 | 70.44 | 228 | 0.002 | 0.403 | 0.201 |
| HR 2135 | Native Am. Vet Mem. (w, b) | 506 | 130.94 | 95 | 0.060 | 0.027 | 0.146 |
| HR 2151 | Merchant marines (w, b) | 169 | 36.57 | 65 | 0.053 | 0.898 | 0.013 |
| HR 2443 | Banking regulation (l, b) | 484 | 141.56 | 330 | 0.002 | 0.154 | 0.714 |
| HR 2602 | Immigrant enforcement (l, b) | 470 | 46.78 | 51 | 0.005 | 0.095 | 0.020 |
| HR 2721 | Worker civil rights (l, b) | 448 | 93.08 | 51 | 0.058 | 0.015 | 0.050 |
| HR 2866 | Forest management (w, b) | 413 | 88.54 | 138 | 0.177 | 0.587 | 0.119 |
| HR 2929 | Budget reform (l, b) | 434 | 27.05 | 179 | 0.363 | 0.011 | 0.066 |
| HR 2947 | War memorial (w, b) | 108 | 42.00 | 60 | 0.074 | 0.000 | 0.070 |
| HR 3342 | Dept. Commerce proj. (w, b) | 271 | 10.48 | 231 | 0.005 | 0.524 | 0.010 |
| HR 3392 | Drinking water (w, b) | 335 | 102.09 | 196 | 0.051 | 0.211 | 0.267 |
| HR 3433 | Calif. Presidio mgmt. (w, b) | 127 | 9.47 | 120 | 0.221 | 0.044 | 0.128 |
| HR 3434 | Smoking restrictions (l, b) | 345 | 57.20 | 74 | 0.081 | 0.050 | 0.050 |
| HR 3546 | Propane education (l, b) | 329 | 101.98 | 122 | 0.051 | 0.126 | 0.133 |
| HR 3600 | Health care reform (l, b) | 328 | 3.31 | 98 | 0.216 | 0.009 | 0.003 |
| HR 3694 | Child abuse (w, b) | 301 | 169.15 | 55 | 0.005 | 0.000 | 0.122 |
| HR 4040 | Worker retraining (l, b) | 212 | 77.71 | 92 | 0.002 | 0.134 | 0.088 |
| HR 4296 | Assault weapon ban (w, b) | 10 | 0.84 | 61 | 0.120 | 0.008 | 0.016 |
| HR 4386 | Veterans' benefits (w, b) | 89 | 40.15 | 162 | 0.016 | 0.285 | 0.108 |
| HR 4570 | Child support (l, b) | 123 | 33.23 | 77 | 0.055 | 0.090 | 0.043 |
| HCR 14 | OSHA (w, n) | 639 | 153.90 | 268 | 0.002 | 0.129 | 0.558 |
| HCR 38 | Embargo of Cuba (l, n) | 605 | 19.94 | 49 | 0.014 | 0.100 | 0.000 |
| HCR 124 | Iranian emancipation (l, n) | 450 | 83.22 | 94 | 0.097 | 0.051 | 0.086 |
| HCR 131 | Sudan (w, n) | 110 | 41.50 | 68 | 0.087 | 0.000 | 0.076 |
| HCR 138 | Saudi Arabia/GATT (l, n) | 396 | 105.43 | 68 | 0.051 | 0.056 | 0.059 |
| HCR 151 | Nigerian democracy (w, n) | 305 | 0.00 | 56 | 0.129 | 0.000 | 0.000 |
| HCR 175 | Arab boycott/Israel (l, n) | 344 | 8.40 | 55 | 0.051 | 0.080 | 0.000 |
| TOTAL | | 337 | 72.18 | 110 | 0.080 | 0.096 | 0.102 |

In the unconstrained model, preference extremity is measured by $p_i^L$ and $p_i^C$, where

$$p_i^L = \begin{cases} 1 \text{ if the legislator's ADA score is in the top quartile,} \\ 0 \text{ otherwise,} \end{cases}$$

and

$$p_i^C = \begin{cases} 1 \text{ if the legislator's ADA score is in the bottom quartile,} \\ 0 \text{ otherwise.} \end{cases}$$

With this information, we estimate by maximum likelihood the dynamic effects of preference extremity, and the static effects of legislator characteristics, on the cosponsorship hazard (see the appendix for the derivation of the likelihood function).[7] Hypothesis tests on these estimates can be used to evaluate empirically the dynamics of cosponsorship behavior. A statistically significant higher cosponsorship hazard among liberals that remains roughly constant over bills' lives, as in Figure 1a, would imply cosponsorship activity commensurate with preferences for the scope of government. A higher cosponsorship hazard for moderates that remains roughly constant over time, as in Figure 1b, would be consistent with position taking, as in our interpretation of electoral-connection theory. Alternatively, a positive and significant effect of preference extremity (for liberals and conservatives alike) that decreases over time would imply an inside-out pattern of cosponsorship, as in Figure 2a, that would be consistent with a model of confirmatory signaling or bipartisan coalition-building.

## RESULTS

Table 3 presents estimates for the constrained model. Results in the first column are based on the full 51-bill sample. Results in the second and third columns are restricted to 29 winning bills and 44 binding bills, respectively.[8]

The first row shows that legislators with longer tenure (House seniority) cosponsor less frequently, conditional on not having cosponsored previously and controlling for electoral margin and preferences. A one-year increase in tenure is associated with a decrease of 0.4% in the hazard rate. Although statistically significant, this effect is substantively small even in the subsample in which the effect is largest. In the subsample of winning bills, the estimate of −0.005 implies that, other things equal, a freshman's cosponsorship hazard is only 10% greater than that of a colleague with 20 years of seniority.

**TABLE 3. Effect of Legislator Characteristics on Cosponsorship Hazard Rates—Constrained Model**
(standard errors in parentheses)

| | All Bills | Winning Bills | Binding Bills |
|---|---|---|---|
| Tenure in house | −0.004<br>(0.002) | −0.005<br>(0.002) | −0.003<br>(0.002) |
| ln (electoral margin) | 0.020<br>(0.014) | 0.022<br>(0.018) | 0.022<br>(0.015) |
| Preference extremity: | | | |
| Early stage | 0.284<br>(0.048) | 0.301<br>(0.064) | 0.271<br>(0.051) |
| Intermediate stage | 0.064<br>(0.045) | 0.069<br>(0.058) | 0.057<br>(0.048) |
| Last stage | 0.071<br>(0.046) | 0.095<br>(0.062) | 0.076<br>(0.050) |

*Note:* For nonpreference characteristics, coefficients are the approximate percentage shift in the hazard rate at all stages of a bill's life. For preference extremity, coefficients are the approximate percentage shift in the hazard rate at a particular stage in a bill's life.

The second row shows that electoral margin has a positive but insignificant effect on cosponsorship regardless of the class of bills considered. This is inconsistent with Campbell's (1982) Senate finding of a negative relationship but consistent with his null House finding. Two differences between these and Campbell's findings should be noted, however. Campbell's dependent variable was the number of bills a legislator cosponsored, and his regression estimates did not consider dynamics of cosponsorship.

The bottom three rows of Table 3 address our two primary concerns: the effect of preference extremity on cosponsorship and on the dynamics of cosponsorship decisions. The positive coefficients indicate that legislators with extreme preferences—defined as those with ADA scores in the top or bottom quartile of the distribution—are statistically significantly more likely to cosponsor, conditional on not having cosponsored, than are those with moderate preferences.[9] This finding is only partially consistent with that in previous studies, because here both extreme conservatism as well as liberalism account for the variation in hazard rates.

The timing of the effect of preference extremity on cosponsorship is the most striking finding in Table 3. The early-stage difference between extremist and moderate hazards is greater than, and statistically distinguishable from, the difference between extremists' and moderates' hazards at both later stages of a bill's life. On the day on which a bill is introduced, legislators with extreme preferences are approximately 28.4% more likely to cosponsor than those with moderate preferences (column 1). For the full sample, this difference is statistically significant at the 0.01 level. During the intermediate and late stages of a bill's life, however, extremists are only 6.4% or 7.1%, respectively, more likely to cosponsor than moderates (conditional on not having cosponsored earlier). This finding persists in subsamples of winning bills (column 2) and binding bills (column 3). Thus, the dynamics of cosponsorship approximate the outside-in pattern graphed in Figure 2a. Extremists from both ends of the ideological spectrum make up a disproportionate number of first-on-board cosponsors, after which this preference-based difference diminishes.

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[7] Conventional maximum-likelihood estimates are obtained under the assumption of the independence of observations, e.g., outcomes of cosponsorship opportunities. This assumption can be questioned. Because cosponsorship opportunities are repeated observations on legislators and on bills, they may not be independent across bills for a given legislator and may not be independent across legislators for a given bill. Future work will investigate the importance of the independence assumption to the estimates.

[8] The class of binding bills excludes resolutions, which merely reflect the sense of the House but do not have the force of law.

[9] Wald tests of the null hypothesis that preference extremity has no effect on the cosponsorship hazard ($\gamma_1^p = \gamma_2^p = \gamma_3^p = 0$) show that the difference between extremists and moderates is statistically significant at $p < .01$ on all three samples of bills ($\chi^2_{(3)} = 38.7$, 26.2, and 31.8 for the full sample, winning bills, and binding bills, respectively).

To assess whether this outside-in dynamic pattern is statistically significant, we consider for each column the differences between the early-stage hazards and the intermediate- and late-stage hazards. For the full sample, a Wald test rejects at a 0.01 level of significance the null hypothesis that the effect of extremism in the early stage equals the effect in the intermediate stage.[10] The result is essentially the same for testing the null that the effect of extremism is the same in early and late stages.[11] Similarly, Wald tests reject the null hypothesis that the influence of preference extremity is equiproportional over time.[12] The significance of this finding is twofold. First, it affirms that the outside-in dynamics of cosponsorship identified by the hazard constitute a robust behavioral pattern that has yet to be detected in large-sample studies. Second, it indicates that a conventional proportional hazards model that constrains the influence of preferences to be proportional over time would be misspecified.

Table 4 presents results from the unconstrained model, which allows the influence of preference extremity on cosponsorship to differ for liberals and conservatives. Again, legislators with extreme preferences generally have higher hazard rates than do their moderate counterparts. Furthermore, for both liberals and conservatives, the outside-in dynamic pattern of cosponsorship for extremists relative to moderates is also similar to that presented in Table 3. In the all-bill sample, for example, extreme liberals have hazard rates that are approximately 44.2% greater than moderates early in a bill's life but only 16.7% greater at the intermediate stage and 13.7% greater at the late stage. Likewise, extreme conservatives have hazards that are approximately 11.7% greater than moderates at the early stage but 3.3% less at the intermediate stage and 1.3% greater at the late stage. As in Table 3, extremist-moderate differences across time are generally statistically significant in Table 4 as well. In the all-bill sample, Wald tests reject the null hypotheses that the effect of extremism in the early stage equals the effect in the intermediate stage, and this is true for both liberals and conservatives, although not significantly true for conservatives at the 0.05 level.[13]

**Table 4. Effect of Legislator Characteristics on Cosponsorship Hazard Rates—Unconstrained Model**
(standard errors in parentheses)

| | All Bills | Winning Bills | Binding Bills |
|---|---|---|---|
| Tenure in house | −0.004 | −0.005 | −0.004 |
| | (0.002) | (0.002) | (0.002) |
| ln (electoral margin) | 0.006 | 0.010 | 0.007 |
| | (0.014) | (0.019) | (0.015) |
| Preference extremity: | | | |
| Liberals | | | |
| Early stage | 0.442 | 0.462 | 0.482 |
| | (0.056) | (0.074) | (0.066) |
| Intermediate stage | 0.166 | 0.130 | 0.170 |
| | (0.057) | (0.076) | (0.069) |
| Last stage | 0.137 | 0.154 | 0.169 |
| | (0.057) | (0.076) | (0.074) |
| Conservatives | | | |
| Early stage | 0.117 | 0.130 | 0.133 |
| | (0.060) | (0.079) | (0.086) |
| Intermediate stage | −0.033 | 0.016 | −0.055 |
| | (0.056) | (0.071) | (0.085) |
| Last stage | 0.013 | 0.043 | 0.113 |
| | (0.057) | (0.076) | (0.084) |

*Note:* For nonpreference characteristics, coefficients are the approximate percentage shift in the hazard rate at all stages of a bill's life. For preference extremity, coefficients are the approximate percentage shift in the hazard rate at a particular stage in a bill's life.

The findings are not perfectly symmetric, however. For liberals, Wald tests reject the null hypothesis that the influence of extremism in the early stage equals the effect in the late stage. This is not true for conservatives at conventional significance levels, however.[14] Thus, the dynamic pattern is somewhat more pronounced on the liberal end of the spectrum than on the conservative end. So, too, is the first-period effect.

Finally, consider the set of null hypotheses that there is no difference between liberal and conservative hazard rates in the early, intermediate, and late stages of bills' lives. These hypotheses can be rejected at a high to modest level of significance.[15] Similarly, a Wald test rejects the joint hypothesis that liberals and conservatives have equal hazard rates throughout a bill's life.[16] Thus, while the outside-in dynamic is indeed two-sided—with a significant conservative as well as liberal first-on-board component—it is also somewhat lopsided, consistent with prior studies' primary or exclusive focus on liberals.

## INTERPRETATIONS

Based on data from the 103rd U.S. House of Representatives, the empirical results indicate that dynamic cosponsorship decisions depend systematically on legislators' characteristics, especially preferences. First, policy extremists on both sides of the political spectrum are more likely than moderates to be early endorsers of legislative initiatives. Second, extremist-moderate differences in cosponsorship behavior are greater in the early stages of a bill's life than in the final stages. These findings have several possible explanations. This section spells out the interpretations and clarifies the range of possible implications of the findings for theories of legislative politics generally and for prior empirical studies of cosponsorship more specifically.

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[10] Testing $h_0$: $\gamma_1^p = \gamma_2^p$, $\chi^2_{(1)} = 11.1$, $p < .01$.

[11] Testing $h_0$: $\gamma_1^p = \gamma_3^p$, $\chi^2_{(1)} = 10.2$, $p < .01$.

[12] Testing $h_0$: $\gamma_1^p = \gamma_2^p = \gamma_3^p$, for all bills, $\chi^2_{(2)} = 14.0$, $p < .01$. Results are comparable for winning bills and binding bills.

[13] $\chi^2_{(1)} = 12.5$ for liberals ($p < .01$) and 3.3 for conservatives ($p \approx .07$).

[14] The test statistics are $\chi^2_{(1)} = 14.8$ for liberals ($p < .01$) and 1.6 for conservatives ($p \approx .21$).

[15] For early, intermediate, and late periods, $\chi^2_{(1)} = 25.3$ ($p < .01$), 9.7 ($p < .01$), and 3.6 ($p \approx .06$), respectively.

[16] The test statistic $\chi^2_{(3)} = 37.6$ is significant at $p < .01$.

As noted in the introduction, the electoral-connection theory views the institution of bill cosponsorship as an opportunity for reelection-seeking legislators to engage in low-cost or costless position taking for which the intended audience is external: constituents and other "relevant political actors" (Mayhew 1974, 39). Mayhew's theory, of course, generates an immense range of empirical implications, spanning individual behavior (position taking, advertising, etc.), legislative organization (committees, rules, etc.), and public policy (particularism, symbolism, etc.). The objective here is not to test the electoral-connection theory generally, and certainly not to challenge the postulate of reelection seeking, but rather to extract and test its specific predictions about dynamics of cosponsorship. While the electoral-connection theory is generally compelling, its more specific hypotheses pertaining to cosponsorship are not supported.

Electoral-connection theory suggests that there should be a close correspondence between legislators' ideological predispositions and the content of the bills legislators choose to cosponsor. The widespread occurrence of heterogeneous first-on-board cosponsors is inconsistent with this expectation. In particular, if moderate bills are more likely to pass than extreme bills, then electoral-connection theory suggests that moderates would be more likely than extremists to cosponsor winning bills. During each time interval, however, moderates are less likely than extremists (of both conservative and liberal types) to cosponsor both winning and losing bills. Electoral-connection theory also suggests that, controlling for preference effects, legislators with uncertain electoral prospects should engage in cosponsorship activity at greater rates than their electorally secure colleagues. The finding of this study, however, is that, other things equal, legislators with low electoral margins are less likely to cosponsor than safe-seat legislators. It bears emphasis that these findings do *not* mean that legislators do not engage in position taking. Rather, it appears that legislators do not use bill cosponsorship as a mechanism for position taking aimed predominantly at external audiences. More generally, the findings are analogous to and consistent with those of Polsby (1969) in his essay on the relative merits of internal versus external leadership strategies.

The findings also suggest some refinements of prior empirical work on bill cosponsorship. Previous studies found some support for the claim that liberals, whose ideology tends to be compatible with big government, cosponsor at higher rates than conservatives (see Campbell 1982, Regens 1989, and Schiller 1995). Although our results provide qualified support for this hypothesis, the qualifications are important. First, the ostensible effect of ideology on cosponsorship in the 103rd Congress is more accurately characterized as an extremism effect than a liberalism effect, since extreme conservatives as well as liberals were more likely than moderates to cosponsor. Second, differences in the estimated effects of ideology on cosponsorship over the course of a bill's life cast doubt on a simple big-government or ideological interpretation of cosponsorship. There is no evidence of a liberal bandwagon, that is, an increase in rates of liberal cosponsorship over time. On the contrary, the difference between liberals' and moderates' cosponsorship behavior diminishes over the course of bill lifespans. Furthermore, this outside-in pattern is also evident—although weaker—on the conservative side of the ideological spectrum. Especially for legislation that is ultimately enacted into law, extreme conservatives, too, are more likely than moderates to cosponsor in the early period, after which the difference between conservatives and moderates diminishes. Thus, although preferences systematically affect cosponsorship, patterns of cosponsorship are not simple instances of big-government liberals endorsing big-government bills.

Young and Wilson (1993) consider cosponsorship as a signaling mechanism, and our findings on the dynamics of cosponsorship serve as an invitation to reconsider theories of legislative signaling. It should be noted that truly dynamic multiperiod formal models of legislative signaling do not exist, although Gilligan and Krehbiel's (1989) heterogeneous committees model might be liberally interpreted as a two-stage, sender-receiver game. In the presence of asymmetric information, heterogeneous specialists (e.g., committee liberals and conservatives) can sometimes credibly transmit valuable information to moderate nonspecialists, thereby facilitating informed decision making by the chamber's median voter. The confirmatory signals sent by heterogeneous committee specialists in the committee-floor context have a straightforward counterpart in cosponsorship analysis. Suppose that first-on-board cosponsors have some private information about the benefits associated with legislation and that they agree on such benefits in spite of their divergent legislative preferences or ideologies. (Optionally but not essentially, suppose further that cosponsorship is a costly act of position taking because, for instance, cosponsors disproportionately bear responsibility for unexpected consequences of legislation that they cosponsor.) Under these conditions and according to the logic of heterogeneous signaling, the prospects of information transmission via cosponsorship are good. Early cosponsorship by heterogeneous legislators may give rise to the sort of outside-in dynamic observed above.

A more diverse set of empirical legislative research also seems broadly consistent with a view of cosponsorship as an intralegislative signaling device. Derthick and Quirk (1985), in their study of deregulation in the 1970s, detail various acts of legislative entrepreneurialism by ideologically diverse legislators. Because Democrats historically had tended to support government involvement in economic activity while Republicans had tended to oppose it, passage of major deregulation legislation was facilitated by—indeed, may have been impossible without—entrepreneurial Democratic committee leaders initiating heterogeneous coalition formation with the assistance of considerably more conservative Republicans. Fenno (1989) describes the surprising success of the Jobs Training Partnership Act as a case in which a key momentum-enhancing event was the joint sponsorship of Dan Quayle (ADA = 18) and Ted Kennedy (ADA = 100). At a press conference announcing this strange-bedfellows partnership, Quayle explained: "Actually, the fact that the two of us would get together underscores the seriousness of the problem of unemployment and emphasizes our commitment" (1989, 50). Jones's (1975) study of environmental legislation in the late 1960s and early 1970s chronicles similar dynamics. In one instance Jones stresses subcommittee expertise as crucial to bill success, citing a heterogeneous bipartisan group composed of Chairman Edmund Muskie (D–Maine), Jennings Randolph (D–West Virginia), Thomas Eagleton (D–Missouri), J. Caleb Boggs (R–Delaware), and Howard Baker (R–Tennessee) (p. 57–8). In a later instance, he quotes Muskie's speech introducing a conference report: "This is not a political measure—it had unanimous support from the Senate conferees—some of whom had differing views on specific language but all of whom knew that a public demand needed a stern response." (p. 208). Krehbiel (1991) explicitly studies legislative signaling theories, presents several specific examples of confirmatory signaling (1991, 84–8), and analyzes systematic evidence pertaining to various stages of the legislative process. For example, he argues that standing committees often possess expertise, are usually initiators in dynamic coalition formation, and are almost always heterogeneous (chapter 4). Furthermore, he presents evidence that minority party cosponsorship significantly increases the likelihood of the adoption of restrictive rules (chapter 5), which in turn facilitates final passage of bills.

Is it valid to interpret outside-in cosponsorship, too, as an instance of heterogeneous signaling? More systematic support for this interpretation can be obtained by ruling out the existence of patterns of cosponsorship in the data that, while plausible, conflict with the interpretation.

Recall that our main finding is that both liberal and conservative extremists are more likely than moderates to cosponsor legislation. Furthermore, we interpret this finding as evidence of heterogeneity of cosponsorship patterns within each bill. Yet, this finding may also be consistent with homogeneity of cosponsors' preferences *within* bills and heterogeneity of preferences *across* bills. This could arise, for example, if the sample were composed of two subsamples of bills: (1) those on which disproportionate early cosponsorship activity comes from extreme liberals and (2) those on which disproportionate early cosponsorship activity comes from extreme conservatives. In other words, cosponsorship could be half left-to-center and half right-to-center rather than all outside-in. Likewise, if (perhaps contrary to intuition) moderate bills were *less* likely to pass than extreme bills, then the finding of disproportionate cosponsorship activity by extremists could be consistent with the predictions of electoral-connection theory, since the sample of bills would consist of two types: (1) those on which disproportionate cosponsorship activity comes from extreme liberals—supposed liberal bills and (2) those on which disproportionate cosponsorship activity comes from extreme conservatives—supposed conservative bills.

These hypothetical scenarios can be investigated with a measure that captures heterogeneity of cosponsorship activity *within* bills. The following index of within-bill dispersion of cosponsors' preferences does this. For each bill $j$ during time interval $t$, let

$$\Phi_{jt} = \frac{\min(N_{jt}^1, N_{jt}^4)}{\max(N_{jt}^1, N_{jt}^4)},$$

where $N_{jt}^1$ ($N_{jt}^4$) is the number of cosponsors on bill $j$ at time $t$ with ADA scores in the first (fourth) quartile of the distribution. The measure $\Phi_{jt}$ is defined on [0, 1] and is increasing in the dispersion of cosponsor preferences at time $t$. Extreme values of the measure have the desired properties. On the one hand, if the number of extreme-liberal cosponsors on bill $j$ at time $t$ (e.g., legislators in the highest ADA quartile) equals the number of extreme-conservative cosponsors (e.g., legislators in the lowest ADA quartile), then $\Phi_{jt} = 1$. This would signify maximal heterogeneity of cosponsors within bills. On the other hand, if all the extreme cosponsors on bill $j$ are either extreme liberals *or* extreme conservatives (but not both), then $\Phi_{jt} = 0$. This would signify artifactual heterogeneity resulting from aggregating distinctly different left-to-center and right-to-center cosponsorship observations.

If the results in Table 3 do not represent genuinely heterogeneous first-on-board cosponsorship decisions on each bill, then the values of $\Phi_{jt}$ in the sample would be close to 0. This is not the case, however. Weighted average values of $\Phi_{jt}$ across bills are consistently high. For the early stage ($t = 1$), $\Phi = 0.740$. For the intermediate stage ($t = 2$), $\Phi = 0.715$. For the late stage ($t = 3$), $\Phi = 0.710$.

We further investigated the validity of the heterogeneous signaling interpretation by reestimating our models after excluding those bills in the lowest quartile of the distribution of $\Phi_j$, where $\Phi_j$ is now defined across all bills without regard to time period. This can also rule out the possibility that the seemingly heterogeneous dynamic results are in fact driven by subsets of bills that are cosponsored by homogeneous legislators. That is, even though the average value of $\Phi_j$ is high (see above), the results could still reflect the presence of bills in the sample with either extreme liberal or extreme conservative cosponsors but not both. Estimates of $\gamma$ coefficients from the basic model do not change substantially after excluding the low-$\Phi$ bills. The estimated effect of preference extremity excluding low-$\Phi$ bills is 0.248 (standard error of 0.054) in the early stage, as compared with an estimated effect of 0.284 (standard error of 0.048) from Table 3. The estimated effect of extremity in the intermediate stage in the restricted sample is 0.034 (standard error 0.050), as compared with an estimated effect of 0.064 (standard error 0.045) from Table 3. And the estimated effect of extremity in the late stage in the restricted sample is 0.054 (standard error 0.051), as compared with an estimated effect of 0.071 (standard error 0.046) from Table 3.

Two substantive conclusions follow. First, there is added within-bill support for the interpretation of the hazard coefficients as heterogeneous first-on-board cosponsorship. Second, there is no hidden support for electoral-connection theory in the form of, say, a balance of extreme liberals cosponsoring liberal bills and extreme conservatives cosponsoring conservative bills.

## FUTURE RESEARCH

On balance, the combination of findings, interpretations, anecdotes, and additional data analysis favors an intralegislative signaling view of the dynamics of cosponsorship rather than the view of cosponsorship as electorally targeted position taking. This study should *not* be interpreted as refutation of electoral-connection theory generally, however, or its claims about position taking outside the domain of cosponsorship. For several reasons, we prefer a more cautious conclusion.

First, additional theoretical work is needed. The most obvious priority is to extend signaling models to settings that more accurately reflect legislators' *dynamic* cosponsorship opportunities. This is likely to be a demanding undertaking if, for instance, one wishes to solve an *n*-person game in which some but not all individual legislators possess and may reveal private information over multiple periods and with multiperiod updating (see Austen-Smith and Riker 1987 for a complex model that has only some of these elements).

Second, additional empirical work is needed to ensure that our conclusions are not peculiar to the 103rd Congress, to Democratic Congresses, to bills with large numbers of cosponsors, or to specific classes of legislation. For instance, future research might distinguish between high- and low-salience bills to see whether different cosponsorship dynamics occur across types. Likewise, tests of alternative claims about other causes of cosponsorship would be useful extensions to these findings.

Third, puzzles persist regarding the fit between extant static theory and these uniquely dynamic findings. For instance, the static and symmetric heterogeneous-committees model cannot explain the asymmetry we observe: a somewhat greater difference between liberals and moderates than between conservatives and moderates. Common sense might attribute this finding to our studying a Democratic Congress whose majority party proposed and endorsed more left-of-center bills than the Republican minority proposed right-of-center bills. If this conjecture is appealing, then two more items should be added to the research agenda. First, one can replicate the study on the 104th Congress, in which Republicans formed a majority that generally sought to enact relatively conservative legislation. Second, if the replication is further supportive of asymmetry, then it would be desirable to have a signaling theory in which asymmetry is derived rigorously as a prediction.

In summary, much remains to be learned about the dynamics of cosponsorship in particular and the dynamics of coalition-building in general. This paper has attempted to further the learning process by showing that hazard analysis is useful for identifying dynamic patterns, by suggesting a class of theory in terms of which the robust pattern of outside-in cosponsorship can be interpreted, and by defining some directions for future theoretical and empirical research on dynamic processes in legislatures.

## APPENDIX: DERIVATION OF THE LIKELIHOOD FUNCTION

The hazard function for bill-legislator combination $i$, which is the probability of cosponsorship at time $\tau$ conditional upon noncosponsorship up to $\tau$, can be written:

$$\lim_{h \to 0^+} \frac{prob(\tau < T_i < \tau + h \mid T_i > \tau)}{h} = \lambda(\tau \mid x_i, p_i, L_i)$$

$$= \lambda_0(\tau, p_i, L_i) \exp(x_i\beta),$$

where $L_i$ is the life of the bill in days, and the baseline hazard $\lambda_0$ and $\beta$ are parameters to be estimated.

The likelihood function of an observation in the constrained semiparametric hazard model is defined as follows (the likelihood in the unconstrained model is derived similarly). Define the cumulative distribution function $F(\tau \mid x_i, p_i, L_i)$ as the probability of cosponsorship after $\tau$ days have elapsed, conditional on observed values of $x_i$, $p_i$, and $L_i$. Then the log of the survivor function, $\log(1 - F(\cdot))$, is related to the integral of the hazard as follows:

$$\log(1 - F(\tau \mid x_i, p_i, L_i)) = -\int_0^\tau \lambda(u \mid x_i, p_i, L_i)du.$$

$F(\tau \mid x_i, p_i, L_i)$ can be rewritten in terms of time intervals $t$:

$$F(t \mid x_i, p_i, L_i) = 1 - \exp\left(-\int_0^{\tau(t,L_i)} \lambda_0(u, p_i)du \cdot \exp(x_i\beta)\right)$$

$$= 1 - \exp\left(-\sum_{j=1}^{t} \int_{\tau(j-1,L_i)}^{\tau(j,L_i)} \lambda_0(u, p_i)du \cdot \exp(x_i\beta)\right),$$

$$= 1 - \exp\left(-\sum_{j=1}^{t} \exp(x_i\beta + \gamma_j + p_i\gamma_j^p)\right),$$

where

$$\gamma_j = \log\left(\int_{\tau(j-1,L_i)}^{\tau(j,L_i)} \lambda_0(u \mid p = 0)du\right),$$

$$\gamma_j^p = \log\left(\int_{\tau(j-1,L_i)}^{\tau(j,L_i)} \lambda_0(u \mid p = 1)du\right) - \gamma_j.$$

Thus, the likelihood of claim $i$ in the basic hazard model (subsuming conditioning on $x_i$ and $p_i$ into a subscript on $F$) can be written:

$$\mathscr{L}(\beta, \gamma) = (F_i(t_i) - F_i(t_i - 1))^{1-I_i}(1 - F_i(3))^{I_i}.$$

where $I_i = 1$ if the legislator did not cosponsor the bill by the end of period 3. Based on this, conventional maximum-likelihood methods are used to obtain estimates of $\beta$ and $\gamma$.

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# The Dynamics of Aggregate Partisanship

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*Despite extensive research on party identification, links between partisanship at the individual and aggregate level have largely been ignored. This leaves a gap in our understanding of the dynamics of aggregate partisanship. To remedy this, we identify a set of ideal types that capture the essential arguments made about individual-level party identification. We then combine the behavioral assumptions for each type with existing results on statistical aggregation to deduce the specific temporal pattern that each type implies for aggregate levels of partisanship. Using new diagnostic tests and a highly general time series model, we find that aggregate measures of partisanship from 1953 through 1992 are fractionally integrated. Our evidence that the effects of a shock to aggregate partisanship last for years—not months or decades—challenges previous work by party systems theorists and students of "macropartisanship." Our arguments and empirical evidence provide a conceptually richer and more precise basis for theories of issue evolution or endogenous preferences—in which partisanship plays a central role.*

An enduring issue within the literature on party identification focuses on the extent to which individual- and aggregate-level measures of party identification exhibit stability, or more accurately persistence or permanence, over time.[1] The recurring nature of this question is due to the centrality of the concept of party identification within the study of politics. Debates about persistence are quite pronounced among those who study micro-level partisanship (e.g., Allsop and Weisberg 1988, Campbell et al. 1960, Converse 1962, Fiorina 1981, Franklin 1984, Franklin and Jackson 1983, Green and Palmquist 1990, Jackson 1975), but questions about its magnitude also arise in analyses of aggregate-level partisanship (e.g., Abramson and Ostrom 1991, 1992; MacKuen, Erikson, and Stimson 1989, 1992; Miller 1991; Weisberg and Smith 1991).

At the individual level, controversies over the degree to which party identification is permanent are stirred by theoretical debates about the formation and causes of change in partisanship. According to traditional theories of individual party identification (e.g., Abramson 1975, 60; Beck 1974, 398; Campbell et al. 1960, 120–5), an individual's party affiliation represents a "lasting attachment" that forms early in life through a process of political socialization. These psychological attachments are expected to persist over time and to work as exogenous influences on voting behavior. Revisionist research (e.g., Achen 1992, 203; Fiorina 1981, 80–90; Franklin 1984, 461; Franklin and Jackson 1983, 958; Jackson 1975, 162; Markus and Converse 1979, 1077–8; Page and Jones 1979, 1059), in contrast, suggests that an individual's party identification is not necessarily permanent but is both a cause of and can be caused by political factors, such as the distance of individual preferences from party policies (Franklin and Jackson 1983; Jackson 1975), candidate evaluations (Page and Jones 1979), prior presidential voting behavior (Markus and Converse 1979), and evaluations of incumbent performance (Fiorina 1981). Yet, Green and Palmquist (1990) call these revisionist claims into question with their empirical evidence that, after accounting for measurement error, political factors have no effect on individual party identification. Green and Palmquist (1990, 872) conclude that "the outstanding characteristic [of party identification] is its persistence over time."

In analyses of aggregate data, the magnitude of persistence in partisanship is also an important issue. For instance, realignment and other party systems theories (e.g., Burnham 1970; Cavanagh and Sundquist 1985; Clubb, Flanagan, and Zingale 1980; Sundquist 1983) are often predicated on an assumption of the durability of individual party identification over long periods. Persistent individual party affiliations are hypothesized to produce a fixed aggregate partisan distribution. Yet, MacKuen, Erikson, and Stimson's (1989) evidence that "macropartisanship" responds to changes in aggregate economic evaluations and approval of the incumbent president (see also Allsop and Weisberg 1988; Weisberg and Smith 1991) fails to support party system theories.[2] In contrast, Miller (1991), who disaggregates the national partisan balance between Republicans and Democrats by region, argues that among whites "the evidence of pervasive, long-term, aggregate stability outside the

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[1] The data and all RATS and MicroTSP computer programs used in this paper may be obtained by anonymous ftp from the Inter-university Consortium for Political and Social Research's Publication-Related Archive at ftp.icpsr.umich.edu/pub/PRA/outgoing/s1119.

[2] MacKuen, Erikson, and Stimson (1989) define macropartisanship as the aggregate percentage Democratic of all party identifiers. They analyze data from 1953 to 1987.

South is so dramatic" that a reexamination of change and persistence in party identification is essential.

The degree of permanence in partisanship has important implications for the way we think about individual partisan attitude change and shifts in aggregate partisanship. For instance, the magnitude of individual partisan persistence has ramifications for explanations of political behavior, such as voting, abstention, and political participation in general. Likewise, the degree of persistence in aggregate partisanship has significant consequences for our interpretations and predictions of local, state, and national election outcomes and trends in public opinion.

Dalton and Wattenberg (1993, 206) note that the extent of persistence in party identification remains an open question. In fact, they suggest that analysts of party identification place this issue at the top of their research agenda. We agree. In addition, we contend that a necessary step toward resolving questions about macro-level partisan persistence involves investigating the relationship between individual- and aggregate-level party identification.[3] Hence, the purpose of our paper is to explore links between individual and aggregate behavior and to use these hypothesized connections to guide our investigation into the magnitude of persistence in aggregate partisanship.

The paper is organized as follows. In part one, we address the question of micro/macro links. To do so, we identify common theoretical assumptions about individual partisan behavior and combine them with results on statistical aggregation to deduce the specific temporal pattern that each set of behavioral assumptions implies for aggregate levels of partisanship. In part two, we test these deductions about aggregate partisanship using diagnostic tests and a general time series model for fractional integration, both of which are new to political scientists. Our use of a time series model that imposes few restrictions on the data allows us to gain leverage over questions surrounding the rate at which shocks to macro-level partisanship dissipate over time. We find that previous conclusions about the degree of persistence in aggregate partisanship must be revised. In part three, we discuss the substantive import of our conclusions and suggest avenues for future research.

## LINKING INDIVIDUAL AND AGGREGATE BEHAVIOR

A key difference between revisionist and traditional accounts of party identification revolves around the specification of causal forces in the individual decision calculus. Franklin and Jackson (1983) argue that the theoretical debates between the two sets of theories can be nested within the following individual decision mechanism:

$$x_{it} = \theta_0 + \alpha_i x_{it-1} + \delta z_{it} + \varepsilon_{it}, \tag{1}$$

where $x_{it}$ is the $i$th individual's current party identification, $x_{it-1}$ is last period's partisan affiliation for the $i$th individual, and $z_{it}$ denotes other systematic factors, such as evaluations of incumbent presidential performance (Fiorina 1981) or evaluations of issue proximity (Jackson 1975) that might influence an individual's party identification. Within the context of equation 1, traditionalists generally assume that $\delta = 0$, so that past party identification is the primary determinant of current party identification. In contrast, revisionists typically assume $\delta \neq 0$, which implies that other systematic factors can cause shifts in party affiliation. Both sets of researchers agree, however, that an individual's party identification is characterized by persistence via "an inertial element . . . that cannot be ignored" (Fiorina 1981, 102).

### Characteristics of Individual-Level Behavior

We begin our analysis by thinking of the extent of permanence in party identification as ranging along a continuum from complete or perfect permanence to no persistence. We then consider a behavioral dimension anchored at opposite ends by the categories of fully homogeneous versus heterogeneous behavior across individuals.[4] Considering these two dimensions jointly results in the four cases of individual behavior depicted in Figure 1. We view these four cases as ideal types or approximations whose purpose is to facilitate our empirical research.[5] Beginning in the upper left-hand corner of Figure 1 and continuing clockwise, let us discuss each case in turn.[6]

***Case 1: Complete persistence with homogeneous behavior.*** Here we consider the situation in which all individuals exhibit completely persistent partisan behavior from one period to the next. Within the context of equation 1, this assumption becomes $\alpha_i = \alpha = 1$. As revisionists, Franklin and Jackson (1983) argue that traditional theories of party identification imply exactly this set of assumptions—complete persistence by all individuals and no effects from short-run factors on party identification. Green and Palmquist's (1990) empirical evidence strongly supports these assumptions.

***Case 2: Less than complete persistence with homogeneous behavior.*** In this case, we emphasize the possibility that individual party identifications exhibit a high but less than complete degree of permanence across all individuals. Relaxing the assumption of complete persistence produces conditions that can be expressed as $0 \leq \alpha < 1$.[7]

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[3] Connections between the two levels of analysis have largely been ignored, although Converse's (1966) discussion of the normal vote contains an attempt at formalizing links between the two. MacKuen, Erikson, and Stimson (1989) also speculate that aggregation gain may explain the linkage.

[4] That is, we take seriously the arguments of Rivers (1988), who suggests that researchers too frequently have assumed that all individuals behave in the same way, and of Achen (1992), who argues assumptions of homogeneity are both highly prevalent and problematic within the literature on individual-level party identification.

[5] Our definition of ideal types relies heavily on Smelser's (1976, 55–6) and Burger's (1976, 133) discussions of Weberian ideal types.

[6] Although it may be possible to identify other ideal types, these four are mutually exclusive and exhaustive with respect to the two dimensions of individual behavior—extent of persistence and degree of homogeneity—that we deem essential to theories of micro-level partisanship.

[7] The existing empirical evidence (e.g., Franklin and Jackson 1983;

**FIGURE 1. Four Ideal Types**

| | | Degree of Permanence | |
|---|---|---|---|
| | | Complete | Less than Complete |
| Degree of heterogeneity | Homogeneous | Case 1<br>$\alpha = 1$ | Case 2<br>$0 \leq \alpha < 1$ |
| | Heterogeneous | Case 4<br>$0 \leq \alpha_i \leq 1$ | Case 3<br>$0 \leq \alpha_i < 1$ |

In this situation, behavior does not vary across individuals, but individual party affiliations are no longer completely permanent. Some prominent theories and analyses that relax the assumption of complete persistence but do not make allowances for differences across individuals include Fiorina (1981), Markus and Converse (1979), Page and Jones (1979), and Weisberg and Smith (1991). Moreover, Achen (1992, 196–7) argues that a majority of the existing empirical work done by traditionalists falls into this category since a homogeneity assumption is built into the linear regression models most traditionalists use to test their hypotheses.

***Case 3: Less than complete persistence with heterogeneous behavior.*** In this case, we continue to focus on situations in which individuals exhibit less than complete partisan persistence, but we now lift the restriction of homogeneous individual behavior. Within the context of equation 1, these assumptions imply that $0 \leq \alpha_i < 1$, thus ruling out complete partisan persistence while allowing for variation across individuals. Research which posits that the degree of partisan persistence varies for those of different ages or with different amounts of political experience (e.g., Beck 1974, Campbell et al. 1960, Converse 1969, Franklin 1984, Franklin and Jackson 1983, Shively 1979) is consistent with these assumptions. Converse's (1962) and Achen's (1992) hypotheses about the way in which partisan persistence varies as a function of new information also fit these assumptions.

***Case 4: Complete persistence with heterogeneous behavior.*** Here we refer to the case in which the degree of partisan persistence varies across individuals over a range that includes instances of complete persistence. Within the context of equation 1, these assumptions can be expressed as $0 \leq \alpha_i \leq 1$. Miller's (1991) empirical evidence that the net balance of party identification for white nonsoutherners is completely persistent, while that for southerners is not, is potentially consistent with these assumptions.[8]

## Implications for Aggregate Partisanship

Notice that in some instances the assumptions associated with each of these four cases fit existing explanations closely, while in others the fit is less tight. Our purpose in forming these ideal types is to use them to shed light on the magnitude of persistence in macrolevel party identification. Testing rival hypotheses about individual-level behavior is not our goal. Instead, we emphasize the way in which individual- and aggregate-level behavior are linked and the way in which these

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Green and Palmquist 1990) suggests that the persistence parameters have a lower bound of zero. Existing theories do not suggest that party affiliations are explosive processes (i.e., $\alpha_i > 1$), so we do not consider such alternatives.

[8] We say "potentially" because, as Miller notes, it could also be the case that countervailing individual shifts in party identification could cancel one another and produce no change in the net balance of party identification.

micro-level assumptions provide insight into the degree of permanence in macro-level party identification.

To formalize the connections between individual- and aggregate-level partisan behavior, we will rely on existing theorems and results on statistical aggregation due to Theil (1954) and Granger (1980).[9] Combining these aggregation results with the assumptions for cases 1 through 4 will allow us to deduce the temporal path that aggregate measures of party identification will follow for all cases. To begin, assume that there are $N_j$ individuals who state that they identify with one of the $j = \{1, 2\}$ major political parties. That is, we want to aggregate separately for Republican and Democratic identifiers. Furthermore, assume that the behavioral decision mechanism for each individual (regardless of party identification) is given by equation 1. Aggregating over the individual equations for one party's identifiers will yield $x_{jt} = \Sigma_{i=1}^{N} x_{it}$, which is an aggregate value of partisanship at a single point in time for the $j$th political party.[10] Performing this aggregation at multiple points for each set of identifiers produces a macro-level time series for each party. Using this approach, we now consider the degree of persistence that we can expect aggregate measures of party identification to exhibit given the behavioral assumptions associated with each ideal type.

## Assumption A: $\delta = 0$

Since the values of $\alpha_i$ provide direct information about the degree of persistence in individual-level partisanship, we will initially assume that $\delta = 0$ in equation 1. In the next section we relax this assumption. By setting $\delta = 0$, we can focus solely on the persistence parameter.[11]

*Case 1.* In this instance behavior is assumed to be homogeneous across all individuals, and the degree of persistence is complete. The assumption that behavior is the same for all individuals allows us to assume that the coefficients in the micro-level equation are constant and independent of $x_{it-1}$, and this makes aggregation straightforward.[12] Granger (1980) and Theil (1954) show that aggregating over the behavior depicted in equation 1, when coefficients are constant, produces a macro-level time series that is *integrated of order one*.[13] Time series that are integrated of order one, denoted I(1), are also called unit root processes. Such time series exhibit long stochastic swings up or down and do not return to a constant mean level. In addition, the effects of a shock to these processes persist permanently.[14]

*Case 2.* In this case, behavior is homogeneous, but the degree of persistence is less than complete. This situation represents a case in which the $\alpha$ coefficient is constant across all individuals, and hence exact aggregation is possible. Under the case 2 assumptions, Theil's (1954) results imply that the aggregate time series will be *integrated of order zero*, denoted I(0).[15] Such time series are also called stationary processes. The effects of a shock to a stationary process are transitory and dissipate quickly over time as the series returns to its constant mean level.

*Case 3.* This case presents a situation in which behavior is no longer homogeneous, and the degree of persistence is less than complete, such that $0 \leq \alpha_i < 1$. Because of the heterogeneity, aggregation becomes more complex. Yet, Granger (1980) proves that, under these circumstances, if $N$ is large and the $\alpha_i$ are drawn from a beta distribution on the range (0,1) with $p,q > 0$,[16] then aggregating over the behavior depicted in equation 1 when $\delta = 0$ will yield a macro-level time series that is *fractionally integrated* (Granger 1980; Granger and Joyeux 1980; Hosking 1981).[17]

Fractionally integrated time series, denoted I($d$) with $0 < d < 1$, differ from stationary processes because they exhibit significant dependence between observations—even those that are separated by very long time spans. Fractionally integrated series also differ from unit root processes because the effects of a shock do dissipate over time as the series reverts to its mean. The rate at which the effects dissipate, however, is slower for a fractionally integrated time series than for a stationary process.

*Case 4.* In this situation behavior is heterogeneous over a range that includes complete persistence. Based on the algebra of integrated series (e.g., Granger and Newbold 1977), Granger (1980) argues that aggregating under these conditions produces a time series that is *integrated of order one*. That is, the aggregate time series is equal to the sum of a fractionally integrated time series (resulting

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[9] A key assumption underlying these theorems is that individuals answer questions about party identification sincerely rather than strategically so that individual decision mechanisms are independent across all individuals. We later relax the independence assumption.

[10] We follow Kramer's (1983) practice of thinking of the aggregation as over a latent, continuous variable ranging from $-\infty$ to $+\infty$ that maps into discretely measured observations depending upon where $x_{it}$ falls with respect to threshold values along the continuum. This latent index tells us about the strength of party affiliation for each individual.

[11] As does Granger (1980), we also set $\theta = 0$, which implies that each aggregate series will contain no drift.

[12] If we believe party identification is measured with random error, then we need to assume $E(\alpha \mid x_{it-1}) = 0$ for all individuals for consistent aggregation (Theil 1954, Rivers 1988).

[13] The intuition behind this aggregation result is that since each individual's behavior is characterized by an autoregressive process with $\alpha_i = 1$, then summing them up and dividing by $N$ produces the same average macro-level relationship.

[14] Additional details about unit root processes can be found in Durr (1993) and Ostrom and Smith (1993).

[15] The intuition here is the same as in case 1: Because each individual's behavior follows an I(0), stationary autoregressive process with the same degree of persistence, so will its macro-level counterpart.

[16] The beta distribution is given by:

$$f(\alpha \mid p, q) = \frac{\alpha^{p-1}(1-\alpha)^{q-1}}{\dfrac{\Gamma(p)\Gamma(q)}{\Gamma(p+q)}}$$

The shape of the distribution depends on the values of $p$ and $q$. When $p = q$, the distribution is symmetric; when $p, q > 1$, the distribution is concave; when $p, q < 1$, the distribution is convex. Given the flexibility of the beta distribution, this assumption seems noncontroversial. In our work, the $q$ parameter indicates how micro-level partisan persistence maps into macro-level persistence. All else equal, the smaller the value of $q$, the larger is the mean of the beta distribution of persistence parameters, and the more persistent will be the aggregate series.

[17] Specifically, $x_t \sim \mathrm{I}(1 - q/2)$, where $q$ is a parameter in the beta distribution. Because the proof of this result is fairly technical, interested readers are referred to the appendix and to Granger (1980).

from aggregation over the individuals for whom persistence is less than complete) and a unit root process (due to aggregation over individuals for whom $\alpha_i = 1$). Because the variance of an integrated series grows as $N$ gets large, the higher order unit root dynamics dominate the lower order dynamics from fractional integration; thus, the resulting macro-level partisanship series will contain a unit root.

## Assumption B: $\delta \neq 0$

The previous aggregate-level predictions were based on the assumption that $\delta = 0$, implying that no political factors or events affect individual party identification or the extent of macro-level persistence. Yet, under the revisionist conceptualization of party identification, individual partisan affiliations are influenced by current political forces. Hence, the question we now address is whether and how the magnitude of persistence in aggregate partisanship may be affected by the features of the $z_{it}$ in equation 1; that is, what linkages arise when we relax the $\delta = 0$ restriction?

To answer this question, consider the following case (Granger 1980):

$$x_{it} = \alpha_i x_{it-1} + \delta z_t + \varepsilon_{it}, \qquad (2)$$

where the $\varepsilon_{it}$ are independent for all individuals, the $z_t$ represent other political factors whose values are taken to be common across individuals, and all else is as defined in equation 1. Technically, the assumption that the $z_{it}$ are common to all individuals implies that $z_{it} = z_t$ for all $i$, which in turn makes the $x_{it}$ dependent across individuals. This assumption allows the properties of the aggregate $z_t$ time series to influence the time series characteristics of the aggregated partisanship series. In addition, this assumption allows factors like unemployment or inflation, battle deaths, or presidential performance ratings to influence an individual's party identification. Given these conditions, we now reexamine our predictions for the degree of persistence in aggregate levels of partisanship.

*Cases 1 and 4.* Granger (1980) argues that, under the assumptions that $\alpha = 1$ and $\delta \neq 0$ (i.e., case 1), aggregating over the behavior in equation 2 continues to yield a macro-level time series that is integrated of order one regardless of the time series properties of the $z_t$ political factors. Similarly, aggregating in case 4 under the assumptions that $0 \leq \alpha_i \leq 1$ and $\delta \neq 0$ will also yield an aggregate partisanship series that contains a unit root. Once again, the intuition is that the unit root dynamics contributed by those individuals for whom $\alpha_i = 1$ dominate the time series properties contributed by the $z_t$.

*Cases 2 and 3.* When we reconsider cases 2 and 3, however, we find that different predictions result. Recall that for case 2, under the assumptions that $0 \leq \alpha < 1$ and $\delta = 0$, aggregate party identification will be stationary. But if $\delta \neq 0$ and the $z_{it}$ are common across individuals and are generated by a process with higher order dynamics (e.g., fractionally integrated or integrated of order one), the resulting macro-level party identification series will be integrated of the same order as the highest order of integration among the components contributing to it (Granger 1980). Thus, in case 2, assuming that $0 \leq \alpha < 1$ and $\delta \neq 0$, aggregate partisanship could be a stationary series, a fractionally integrated process, or a nonstationary unit root process. Finally, consider case 3 when the assumptions $0 \leq \alpha_i < 1$ and $\delta \neq 0$ hold. If we continue to assume that the $\alpha_i$ follow a beta distribution on (0,1), then the aggregate partisanship series, $x_t$, will be dominated by the higher degree of integration contributed by either the $z_t$ or $\varepsilon_{it}$ (Granger 1980).[18] Hence, for case 3 under assumption B, macro-level party identification could be either fractionally integrated or integrated of order one.[19]

**FIGURE 2. Predicted Dynamic Properties of Aggregate Partisanship**

| | Assumption A<br>$\delta = 0$ | Assumption B<br>$\delta \neq 0$ |
|---|---|---|
| Case 1<br>$\alpha = 1$ | Unit Root | Unit Root |
| Case 2<br>$0 \leq \alpha < 1$ | Stationary | Stationary, Unit Root, or Fractionally Integrated depending on dynamics of $z_t$ |
| Case 3<br>$0 \leq \alpha_i < 1$ | Fractionally Integrated | Unit Root or Fractionally Integrated depending on dynamics of $z_t$ |
| Case 4<br>$0 \leq \alpha_i \leq 1$ | Unit Root | Unit Root |

By combining assumptions about individual behavior with existing aggregation results we have deduced the temporal path of aggregate partisanship for each of the four ideal types under two different assumptions about the effects of other exogenous political factors. Figure 2

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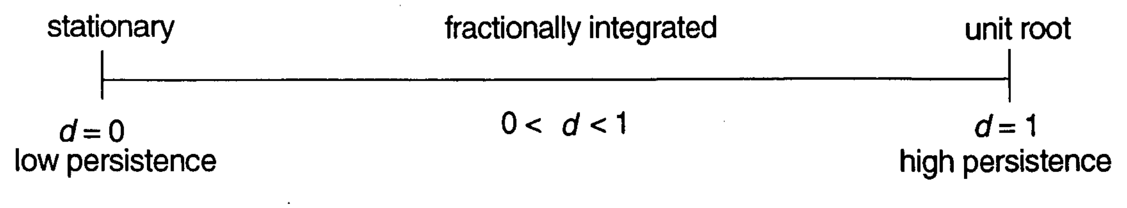
[18] These degrees of integration are denoted $I(d_z + 1 - q)$ and $I(1 - q/2)$, respectively.

[19] When other exogenous factors that are independent across individuals, say, $w_{it}$, are included in equation 2, Granger (1980) shows that $x_t$ will be $I(d_x)$, where $d_x$ is the largest of three components: $1 - q/2 + d_w$ (from the $w_{it}$), $1 - q + d_z$ (from the $z_t$), and $1 - q/2$ (from the $\varepsilon_{it}$).

**FIGURE 3. Persistence Properties of Time Series**



summarizes our predictions. Here we see that evidence of stationarity would be consistent only with the case 2 assumptions. Evidence of fractional integration would be consistent with the case 3 assumptions or with the case 2 assumptions when $\delta \neq 0$. Finally, empirical evidence of a unit root would be consistent with cases 1 and 4 or with cases 2 and 3 when $\delta \neq 0$. Although these predictions for macro-level partisanship overlap in some instances, empirical tests of the degree of persistence in aggregate party identification can, nonetheless, provide us with important insight into the nature of macro-level partisan persistence and its links to individual-level heterogeneity and permanence.

## TESTING THE ALTERNATIVE PREDICTIONS

From a technical viewpoint, distinguishing among the predictions in Figure 2 requires that we estimate the degree of persistence in aggregate partisanship. Figure 3 shows the empirical patterns of persistence associated with various types of time series. For a stationary series, shocks will show little persistence, while shocks to a first-order autoregression process [i.e., AR(1)] with a unit root will exhibit high levels of persistence. For fractionally integrated time series, the degree of persistence varies such that the higher the value of *d*, the greater the degree of persistence. To investigate the degree of persistence in aggregate partisanship and test the predictions in Figure 2, we use two approaches. First, we employ diagnostic tests of the general class into which a time series falls (i.e., unit root, variance ratio, and KPSS tests). Second, we use a maximum likelihood estimator to obtain point estimates of *d*, the order of integration. These point estimates provide direct information about the degree of persistence.

The fact that we focus on empirical estimates of the degree of persistence in aggregate partisanship becomes very important once we take up issues of measurement. We measure aggregate levels of partisanship from the first quarter of 1953 through the fourth quarter of 1992 as the percentage of Republican identifiers, the percentage of Democratic identifiers, and the Democratic percentage of the major party identifiers. This latter measure is MacKuen, Erikson, and Stimson's (1989) "macropartisanship" series.[20] That is, we use percentages, which are bounded by 0 and 100, as proxies for the aggregate number of party identifiers.[21] Some analysts question the meaningfulness of unit root tests in bounded data. Since unit root processes have asymptotically infinite conditional variances, these researchers argue that we cannot expect a bounded variable to have a unit root (DeBoef and Granato n.d.; Williams 1993).

In our analysis, however, we emphasize the degree of persistence in a finite sample rather than the asymptotic distribution of the OLS estimator; hence, the observations of Hamilton (1994) apply. As Hamilton points out, when the question is one of persistence, unit root tests are indeed relevant. According to Hamilton (1994, 447), "we can measure whether innovations have much persistence over a fixed interval [with a unit root process as the true model] or very little persistence over that interval [with a stationary model as the true model]." Thus, we contend that, even for bounded data, unit root tests provide meaningful information about the degree

**TABLE 1. Dickey-Fuller Tests for Unit Roots**

| | DF | ADF[a] | Joint Test |
|---|---|---|---|
| % Republican | −2.475 | NA | 3.175 |
| % Democrat | −1.942 | −1.130 | 2.821 |
| Macropartisanship | −2.198 | −1.598 | 2.423 |
| Critical values: | | | |
| $\alpha = .01$ | −3.47[b] | −3.47 | 8.43[c] |
| $\alpha = .05$ | −2.88 | −2.88 | 6.34 |
| $\alpha = .10$ | −2.58 | −2.58 | 5.39 |

Null hypothesis for DF/ADF tests: unit root process, $d = 1$
Alternative hypothesis: stationary process, $d = 0$

Null hypothesis for joint test: unit root process with no linear trend
Alternative hypothesis: stationary process

[a] When we detected autocorrelation in the residuals from the DF regression, we estimated ADF regressions using enough lagged first differences to make the residuals white noise.

[b] The DF and ADF statistics have a nonstandard distribution. The appropriate critical values (shown above) are given in MacKinnon (1991).

[c] The statistic for the joint hypothesis has a nonstandard distribution. The appropriate critical values (shown above) are given in Dickey and Fuller (1981).

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[20] The macropartisanship series ends with the second quarter of 1992. We thank Jim Stimson for these data. The macropartisanship series has been corrected for potential biases from telephone polling, while the others have not. These biases could make long-run persistence more difficult to detect. All data are taken from responses to the Gallup Poll's party identification question. The summary statistics for each series are: % Republican—mean = 27.90, std. dev. = 3.92, $N$ = 160; % Democratic—mean = 42.90, std. dev. = 5.42, $N$ = 160; macropartisanship—mean = 61.34; std. dev. = 4.68; $N$ = 158.

[21] In the best case scenario, we would want to use the number of people identifying with a political party. These numbers could grow and decay over time. Unfortunately, the only data available are from samples of individuals, and hence we must use percentages to account for varying sample sizes. These percentages can grow or decay but are admittedly bounded by 0 and 100.

**TABLE 2. Variance Ratio Tests**

| | VR (*k*) | | | | |
|---|---|---|---|---|---|
| | VR(2) | VR(4) | VR(8) | VR(16) | VR(32) |
| % Republican | 1.212 | 1.593 | 1.791 | 2.650 | 2.051 |
| % Democrat | 1.508 | 1.845 | 2.549 | 2.997 | 3.033 |
| Macropartisanship | 1.159 | 1.360 | 1.516 | 2.062 | 1.872 |
| Critical values: | | | | | |
| $\alpha = .05$ | 1.157 | 1.328 | 1.609 | 2.170 | 3.578 |
| $\alpha = .10$ | 1.122 | 1.254 | 1.465 | 1.891 | 2.909 |

Null hypothesis: random walk with drift, $d = 1$
Alternative hypothesis: pure fractional noise, $d < 1$

Recommended differencing interval (Diebold 1989:) $k = 4$ or $k = 8$

of persistence in a time series, helping analysts determine where a series falls along the continuum in Figure 3. In addition, we rely on multiple tests of persistence, which help to ensure that our conclusions are robust.

## Results of Diagnostic Tests

We begin our empirical analysis by using diagnostic tests to investigate the time series properties of the aggregate partisanship data. Although these tests cannot always pinpoint the order of integration for a time series, the evidence they provide can be used to determine whether a time series is "closer" to being stationary, with $d = 0$ and low persistence, or to exhibiting unit root behavior, with $d = 1$ and high persistence. In general, we agree with Lo (1991, 1296) that these tests can be used to complement evidence obtained from point estimates of $d$. We employ unit root, variance ratio, and KPSS tests as our diagnostic tools.[22]

Table 1 presents the results of the conventional Dickey-Fuller (DF) tests for unit roots. As the results in column 1 show, the null hypothesis that each of these series has a unit root cannot be rejected. As shown in column 2, this conclusion continues to hold when Augmented Dickey-Fuller (ADF) tests are applied to the macropartisanship and Democratic series.[23] Furthermore, a joint test of the null hypothesis of a unit root but no time trend indicates that these series are not deterministic functions of time. To summarize, the evidence in Table 1 suggests that each of these series exhibits unit root behavior and is highly persistent.

Dickey-Fuller tests, however, have low power in the face of fractionally integrated alternatives.[24] To compensate for this, we also used variance ratio tests (Cochrane 1988; Diebold 1989) of the null hypothesis of a random walk with drift, i.e., $d = 1$, versus an alternative of pure fractional integration with $d < 1$. The results of these variance ratio tests appear in Table 2 for various choices of $k$, the differencing interval. As these results show, the null hypothesis of a unit root is rejected for both the Republican and Democratic series when $k = 2, 4, 8$, and 16. When applied to macropartisanship, the variance ratio test rejects the null hypothesis at the $\alpha = 0.05$ level for $k = 2$ and 4 and at the $\alpha = 0.10$ level for $k = 2, 4, 8$, and 16.[25] On the whole, the evidence in Table 2 suggests that each of these three series is fractionally integrated with $d < 1$ and with a degree of persistence that varies depending upon the value of $d$. This implies that aggregate partisanship exhibits a lower degree of persistence than is found in an AR(1) process with a unit root.

Finally, in Table 3, we present the results from KPSS tests of the null hypothesis that each series is a strong mixing process.[26] We present these results for various choices of $\ell$ (lag truncation parameter), which affects the number of lagged autocovariances used to compute the long-run variance of the series.[27] As the empirical evidence in Table 3 shows, the null hypothesis that these series are strong mixing can be rejected at the $\alpha = 0.05$ level for all values of the lag truncation parameter from 0 through 6. These results suggest that these partisanship time series are not stationary processes and that $d > 0$ for each. This implies that aggregate partisanship exhibits a higher degree of persistence than is found in a stationary AR(1) process.

Taken together, the evidence in tables 1 through 3

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[22] Since unit root tests are only one of three diagnostic tests that we employ, and since we estimate the order of integration directly, the consistent empirical results obtained from these multiple tests suggests that our conclusions are robust to the type of test employed.

[23] Dickey-Fuller tests are biased when the residuals are serially correlated. When we detected autocorrelation in the residuals of the DF regressions, we estimated ADF regressions using enough lagged first differences to make the residuals white noise.

[24] DeBoef and Granato show that DF tests also have low power when the alternative hypothesis is one of near integration. See DeBoef and Granato (n.d.) for more information on near-integrated time series.

[25] Diebold's (1989) simulations suggest that for our sample size, the variance ratio test is most powerful when $k = 4$ or $k = 8$. Diebold's Monte Carlo results show that when $d = 0.70$, the power of the test is 0.889 for a 95% confidence interval and is 0.95 for a 90% confidence interval. When $d = 0.80$, the power of the test is 0.56 for a 95% confidence interval and is 0.70 for a 90% confidence interval.

[26] Intuitively, a time series is strong mixing if the rate at which dependence between past and future observations goes to zero as the distance between them grows is "fast" enough (Lo 1991). Stationary series, which decay at a geometric rate, are strong mixing processes; fractionally integrated series, which decay at a hyperbolic rate, and unit root processes, which do not decay, are not strong mixing. Hence, determining whether aggregate party identification measures are strong mixing provides general information about rate of decay and degree of persistence.

[27] Simulations by Kwiatkowski et al. (1992) show that $\ell = 4$ provides the most powerful test, given our sample size. Their Monte Carlo results show that when $\ell = 4$, the power of the test is greater than or equal to 0.92 for a 95% confidence interval in all typical circumstances.

**TABLE 3. KPSS Tests for Strong Mixing**

| | Lag Truncation Parameter ($\ell$) | | | | |
|---|---|---|---|---|---|
| | 0 | 2 | 4 | 6 | 8 |
| % Republican | 2.852 | 1.011 | .629 | .465 | .374 |
| % Democrat | 8.784 | 3.112 | 1.932 | 1.425 | 1.145 |
| Macropartisanship | 3.159 | 1.124 | .706 | .529 | .431 |
| Critical values: | | | | | |
| $\alpha$ = .01 | .739 | .739 | .739 | .739 | .739 |
| $\alpha$ = .05 | .463 | .463 | .463 | .463 | .463 |
| $\alpha$ = .10 | .347 | .347 | .347 | .347 | .347 |

Null hypothesis: strong mixing process
Alternative hypothesis: not strong mixing

Recommended lag truncation (Kwiatkowski et al. 1992): $\ell = \text{int}[4(T/100)^{.25}] = 4$

suggests that aggregate measures of Republican and Democratic identifiers exhibit neither stationary nor unit root behavior. Similarly, the macropartisanship series is not stationary. In the case of macropartisanship, however, the evidence against the unit root hypothesis is not quite as strong as it is for the other two series. The pattern of hypothesis rejections for the Republican and Democratic time series is consistent with the conclusion that these series are fractionally integrated. The evidence for macropartisanship suggests that this series is fractionally integrated, although high persistence and unit root behavior cannot be ruled out conclusively.

## Point Estimates of the Order of Integration

While they are useful tools, these diagnostic tests do not allow us to draw precise inferences about the degree of persistence in aggregate partisanship. To determine which of the predictions in Figure 2 is consistent with the aggregate-level evidence, we need point estimates of *d*, the order of integration, for each of these time series. To obtain estimates of *d*, we use the full information maximum likelihood (ML) estimator derived by Sowell (1992a, 1992b).[28]

The ML estimates of *d* appear in Table 4.[29] Here we see that each of the three partisanship series is fractionally integrated, since for each series the hypothesis that $d = 0$ (i.e., stationarity) as well as the hypothesis that $d = 1$ (i.e., unit root) can be rejected at conventional levels of statistical significance.[30] In addition, we see that each of these partisanship series exhibits a high degree of persistence, since the estimates of *d* are 0.804 for the Republican series, 0.698 for the Democratic series, and

**TABLE 4. Maximum Likelihood Estimates of Degree of Fractional Integration (*d*)**

| | Parameter Estimates[a] |
|---|---|
| % Republican | .804[b]<br>(.057) |
| % Democrat | .698<br>(.058) |
| Macropartisanship | .839<br>(.064) |

[a] The standard errors of the estimates are shown in parentheses.
[b] The ML "t-ratios" for tests of the null hypothesis that $d = 1$ are −3.44, −5.21, and −2.52 for the Republican, Democratic, and macropartisanship series, respectively. The ML "t-ratios" for tests of the null hypothesis that $d = 0$ are 14.12, 12.03, and 13.11 for the Republican, Democratic, and macropartisanship series, respectively.

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[28] Sowell's (1992a) FIML estimator of *d* is always applied to first-differenced data so that stationarity is forced to hold. Then assuming that $X_T = \{x_1, x_2, \ldots, x_T\}$ is a normally distributed sample of *T* observations, the likelihood function of the ARFIMA(*p,d,q*) process is given by:

$$L(X_T|\Sigma) = (2\pi)^{-T/2}|\Sigma|^{-1/2}\, e^{(-1/2 X'\Sigma^{-1}X)}$$

where $\Sigma$, the $T \times T$ Toeplitz covariance matrix of $X_T$, is a function of $d, \phi_1, \ldots, \phi_p, \theta_1, \ldots, \theta_q$, and $\sigma^2$. This function must be maximized with respect to $\Sigma$, *d*, and the *p* autoregressive and *q* moving-average parameters. To do so, the spectral density of $x_t$ needs to be written in terms of the model parameters, and then the autocovariance function is calculated by:

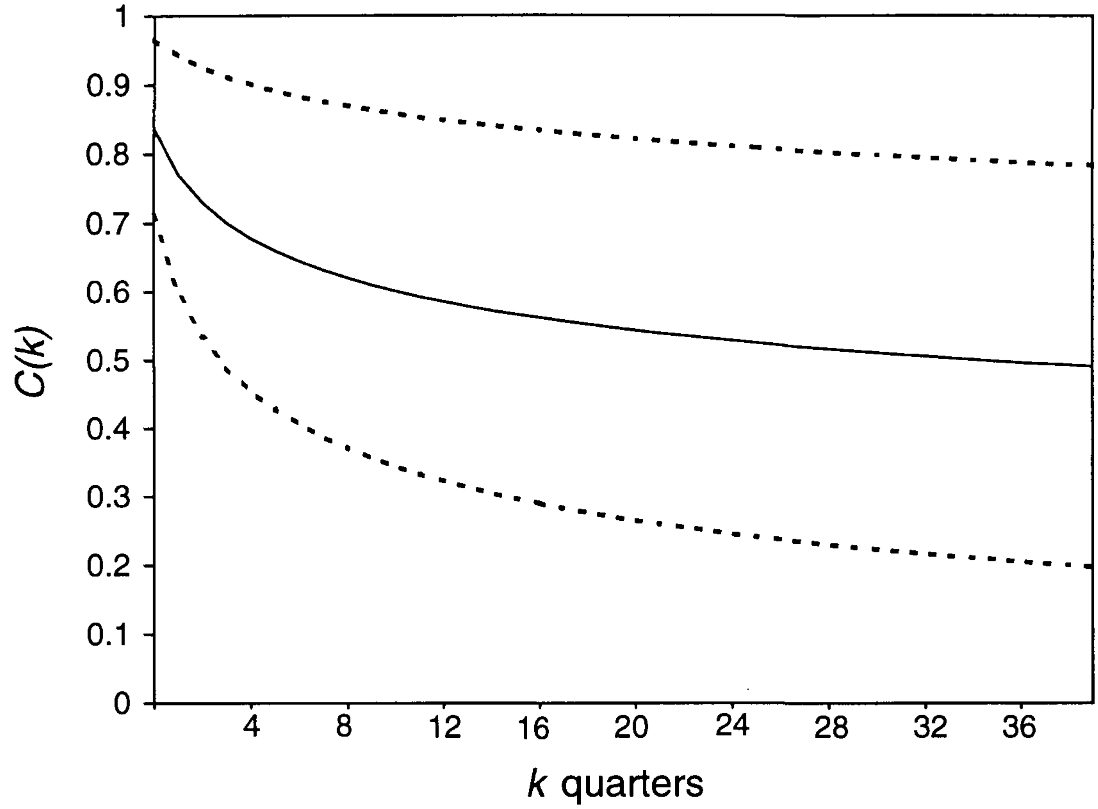
$$\gamma(s) = \frac{1}{2\pi}\int_0^{2\pi} f_x(\lambda)e^{i\lambda s}\, d\lambda$$

which Sowell proves is well approximated for various lag lengths by sums of a hypergeometric function. Thus, the assumption of a beta distribution never enters into the likelihood but is used only to establish micro/macro links.

[29] We also estimated *d* using a popular nonparametric, spectral regression-based procedure, called the GPH estimator after its developers, Geweke and Porter-Hudak (1983). Our GPH results indicate that for each series the null hypothesis of stationarity can be rejected, but the GPH estimates of *d* cannot be statistically distinguished from a value of 1.0 due to limited degrees of freedom and large standard errors. Sowell's (1992a) simulations show that his ML estimator generally has less bias and a smaller MSE in small samples than does the GPH estimator. Hence, we use Sowell's time domain ML estimator.

[30] It is possible for a time series to be fractionally integrated (FI) and to contain short-run autoregressive (AR) or moving average (MA) components. Such series are called ARFIMA(*p,d,q*) processes. The results in Table 4 are based on estimation of ARFIMA(0,*d*,0) models with no AR or MA components. To test our restrictions, we estimated ARFIMA(*p,d,q*) models with up to three AR and three MA parameters each. Based on likelihood ratio tests comparing the various unrestricted models to the restricted model, we conclude that the restricted ARFIMA(0,*d*,0) cannot be rejected.

**FIGURE 4. Impulse Response Function for Macropartisanship Series**



0.839 for the macropartisanship series.[31] The closer the absolute value of *d* to 1, the more persistent are the effects of shocks to these series.

## The Magnitude of Aggregate Persistence

Additional insight into the degree of permanence in macro-level partisanship can be obtained by examining the cumulative impulse response function for each series.[32] By considering the entire sequence of cumulative impulse responses, we can investigate the effect on aggregate partisanship of a shock to each series after one quarter, one year, one presidential administration, or even one hypothesized party system period.

The cumulative impulse response functions, denoted $C(k)$, and their associated confidence intervals are presented separately for each partisanship series in figures 4 through 6.[33] In these figures, the solid lines indicate the degree to which the effect of a one-standard-deviation shock to each series persists over time.[34] For instance, consider the response function for the macropartisanship series shown in Figure 4. Here we find that the degree to which a shock persists drops from 0.84 to 0.70 after four quarters (one year). After 16 quarters (four years), the degree of persistence has only been cut by about one-third, since $C(16) = 0.57$. In fact, as the confidence intervals make clear, the degree of persistence in macropartisanship remains statistically different from zero even after 40 quarters (ten years). As shown in figures 5 and 6, similar patterns of persistence are found in the Republican and Democratic series.[35] The slow decay of these cumulative impulse response functions

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[31] The ML estimates were obtained through the Davidon-Fletcher-Powell algorithm. We follow Sowell (1992a) and use the GPH estimates of *d* as our starting values. We checked numerous other values, however, and found that our ML estimates of *d* are highly robust to the starting value used. The standard errors of *d* for the Republican and macropartisanship series are robust to the starting value used for *d*, although the standard error of *d* for the Democratic series varied somewhat when starting values near zero were used. To ensure we had convergence of the standard error of *d*, we restarted the DFP algorithm at exactly the point it stopped and ran additional iterations.

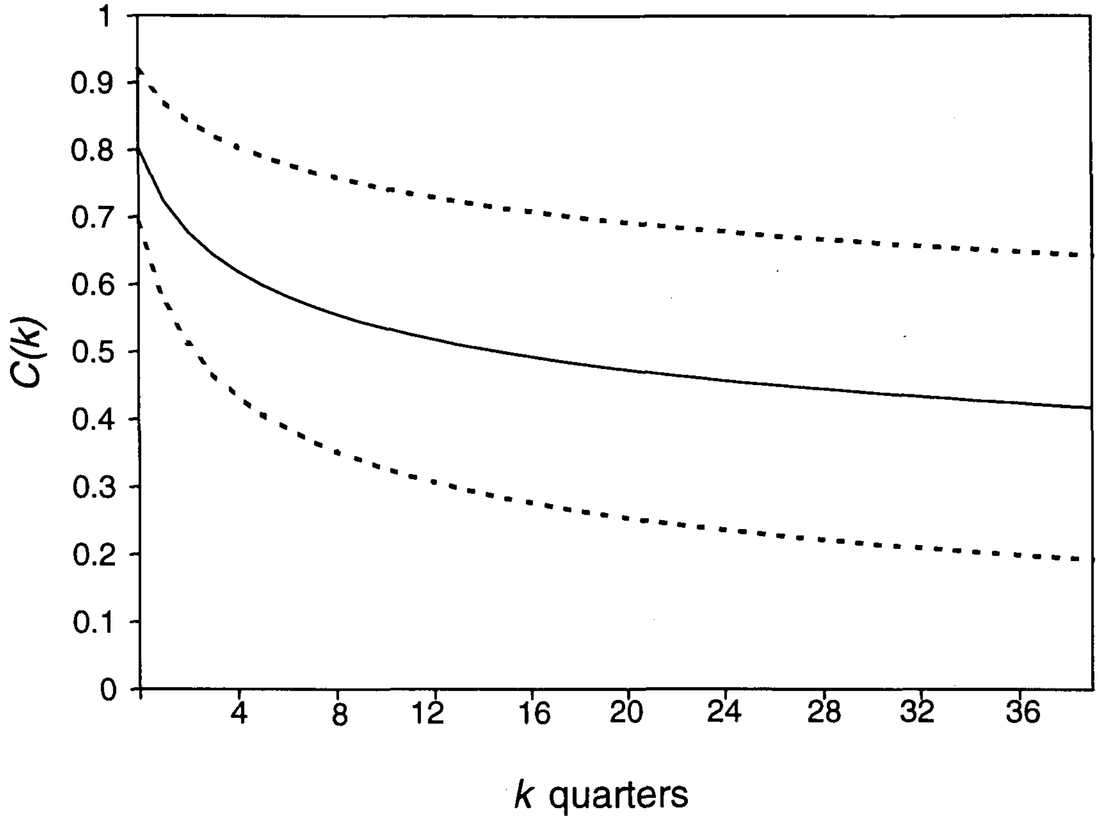
[32] The cumulative impulse response function represents the sum of the coefficients of the moving-average lag-operator polynomial of the differenced series. See Diebold and Rudebusch (1989) for a useful discussion of the impulse response function for fractionally integrated time series.

[33] The confidence intervals (represented by dashed lines) are constructed from ±2 standard errors of $C(k)$. As Hamilton (1994, 336–40) points out, there are alternative approaches to calculating these standard errors, especially for the orthogonalized impulse response function of a vector autoregression (e.g., Runkle 1987). Since we are working in the univariate context, many of these approaches are not relevant to our work. Hence, we follow Diebold and Rudebush (1989) and Hamilton (1994) and use a numerical approach for nonorthogonalized impulse responses to calculate the standard errors.

[34] More precisely, these figures show the effect of a one-standard-deviation shock to the level of each partisanship series for up to *k* future periods, where *k* ranges from 1 to 40 quarters (see also Cheung 1993).

[35] We note that the degree of persistence in the Democratic series is significantly different from the extent of permanence in the other two series. Substantively, this implies an asymmetry in the degree of partisan persistence for Democrats and Republicans. This, in turn,

**FIGURE 5. Impulse Response Function for Republican Series**



indicates that increases and decreases in aggregate levels of partisanship are "permanent" in the sense that they last for years.[36]

## Ruling out a Rival Hypothesis

While we have focused on individual-level heterogeneity and the features of the $z_{it}$ political factors as the sources of the dynamics of aggregate partisanship, it is also important to rule out the hypothesis that generational replacement is responsible for the high degree of persistence found in our data.[37] The issue is whether population changes affect the distribution of new entrants' party identifications such that these distributions change over time. For instance, there is some evidence that individuals who entered the electorate in recent years are more Republican than in previous years (Abramson, Aldrich, and Rohde 1995).

Whether such population changes are the source of the dynamic patterns of fractional integration in the aggregate partisanship data is an empirical question that can be addressed by dividing the data into subsamples and reestimating the persistence parameter, $d$. For instance, in case 3, under the assumption that $\delta = 0$, the estimated value of $d$ is mathematically related to the mean of the beta distribution of $\alpha$ such that the higher the estimate of $d$, the higher is this mean. In all cases, stability of the estimates of $d$ across periods is inconsistent with the hypothesis that population changes are the primary cause of the degree of permanence in aggregate measures of party identification.

To test the stability of the coefficient estimate of $d$ for the macropartisanship series, we used subsamples of approximately two decades in length, one from the second quarter of 1953 through the fourth quarter of 1972 and the other from the first quarter of 1973 through the second quarter of 1992. Our choice of these subsamples was based on Abramson's (1983, 54) observation that "during the postwar era, nearly half the white electorate has been renewed every two decades." The estimate of $d$ for macropartisanship during the period from the second quarter of 1953 to the fourth quarter of 1972 is 0.746, while the estimate for the period from the first quarter of 1973 to the second quarter of 1992 is 0.953. Each of these coefficient estimates falls into the 95% confidence interval of (0.714, 0.964) around the original estimate of $d$ = 0.839 for the entire sample.[38] The fact that these coefficients are not statistically different from each other suggests that they are stable during periods of population change, and this evidence

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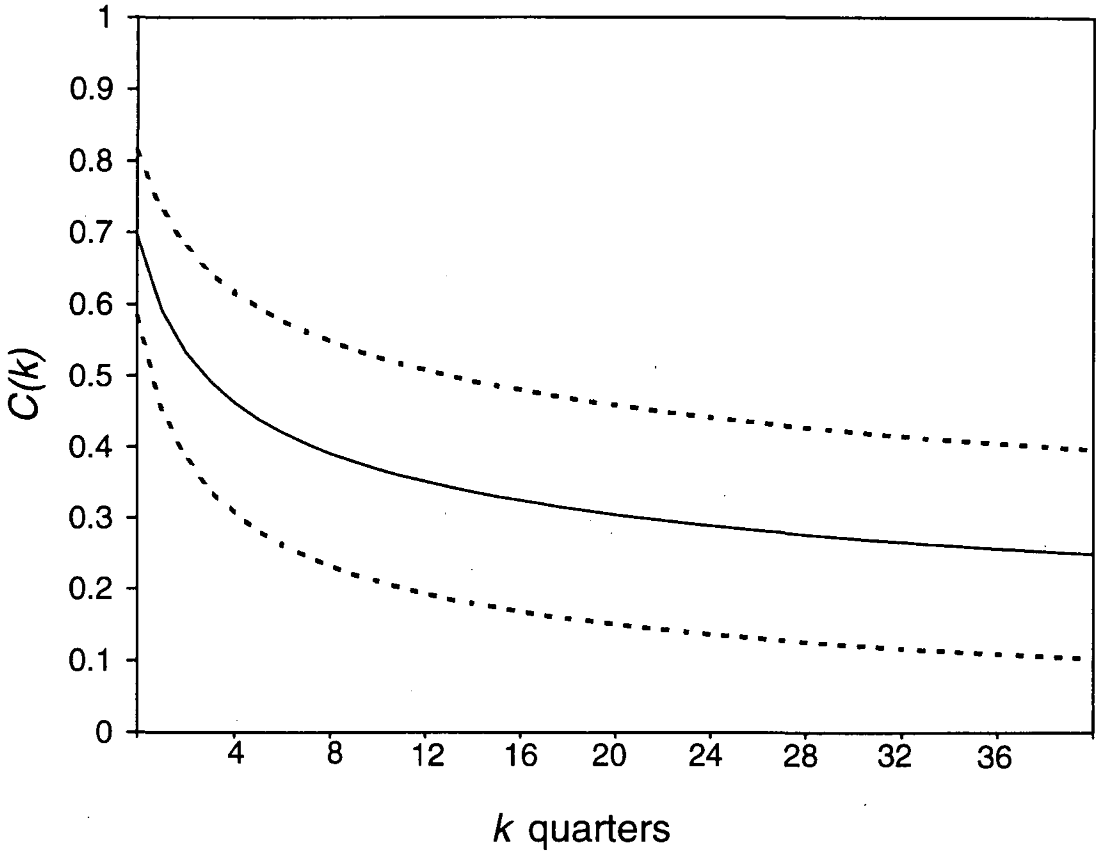
could imply that the individual-level sources of heterogeneity exert a slightly different influence on Democrats than on Republicans.

[36] The degree of permanence in aggregate partisanship continues to weaken after the ten-year period. For instance, after twenty years, $C(k) = 0.365$ for the Republican series, $C(k) = 0.204$ for the Democratic series, and $C(k) = 0.439$ for the macropartisanship series.

[37] We thank a reviewer for raising this point.

[38] The results for Democratic and Republican identifiers are similar—0.713 and 0.677 for Democrats and 0.560 and 0.884 for Republicans.

FIGURE 6. Impulse Response Function for Democratic Series



refutes the idea that such changes are the primary cause of aggregate partisan persistence. We thus reject this rival hypothesis.

## DISCUSSION AND IMPLICATIONS

The empirical evidence in tables 1 through 4 can be used to resolve certain questions about the nature and degree of persistence in aggregate-level party identification and its links to individual partisanship. The evidence shows that aggregate levels of partisanship are fractionally integrated. Since this empirical result disconfirms the predictions associated with cases 1 and 4, we can conclude that theories in which all individuals are assumed to maintain completely persistent party identifications provide an inadequate micro-level explanation of the dynamics of aggregate party identification. We also see that the stationary and unit-root predictions from case 2 and the unit-root prediction from case 3 fail to be supported by the empirical evidence.

Instead, our evidence that aggregate measures of party identification are fractionally integrated is consistent with only two instances—case 3, in which individuals exhibit heterogeneous and incompletely persistent party identifications, or case 2, in which individuals exhibit homogeneous behavior and less than complete persistence when $\delta \neq 0$ and the $z_t$ follow a fractionally integrated process. In case 3, the persistence in macro-level partisanship is solely a function of individual-level heterogeneity. In case 2, the degree of macro-level persistence can be a function of an aggregate $z_t$ series that is fractionally integrated or of a mixture of individual-level heterogeneity and long memory time series properties for $z_t$.

### Implications for Micro-Level Party Identification

Our evidence of fractional integration in aggregate levels of party identification along with our discussion of potential micro/macro links help us refocus the attention of analysts of individual party identification on the issues of the heterogeneity of the $\alpha$ coefficients and on the degree of persistence expected over time in the exogenous $z_{it}$ factors. Given our evidence, analysts of micro-level party identification must now ask such questions as: Are issue proximities persistent enough to cause the kind of aggregate-level permanence we find? Do evaluations of the incumbent president's performance contain a permanent component that would explain the fractionally integrated dynamics of aggregate partisanship? Furthermore, these researchers must open their theoretical and empirical analyses to the possibility of behavioral heterogeneity.

Finally, it seems most likely that the above issues cannot be investigated separately. Instead, future research into individual-level party identification will need to specify both the nature of behavioral heterogeneity (through the $\alpha_i$) and the dynamic patterns of any political factors (the $z_{it}$) thought to influence party identification. Only those researchers who do so will be able to provide a solid microfoundation for aggregate partisanship.

## Implications for Aggregate-Level Partisanship

Our empirical evidence that aggregate measures of party identification as well as MacKuen, Erikson, and Stimson's (1989) macropartisanship series are fractionally integrated also has serious implications for conclusions about the magnitude of persistence in macro-level party identification. Based on their analyses, MacKuen, Erikson, and Stimson (1989, 1139) reject the long-cycle hypothesis of party system theorists and conclude that partisan "gains and losses are 'permanent' on a scale of months, not decades."

But as shown in figures 4 through 6, our analysis sheds new light on the nature of the "mid-range dynamics" of aggregate partisanship.[39] As the evidence shows, more than one-half the effects of any shock to aggregate partisanship persist for as long as a four-year presidential term. More than one-quarter of the effects remain after eight years. Thus, based on our evidence, we also reject the party system theorists' arguments about decades-long cycles of partisanship. But, in contrast to MacKuen, Erikson, and Stimson (1989), we conclude that shocks to aggregate partisanship are "permanent" on a scale of years—not months.

The disparity between our conclusions and those of MacKuen, Erikson, and Stimson arises from their assumption that macropartisanship is stationary with $d = 0$. Our evidence shows that the stationarity hypothesis can be rejected in each of our three aggregate time series. Because MacKuen, Erikson, and Stimson (1989) impose the restriction $d = 0$ in their statistical analyses, they automatically constrain the rate of partisan decay to the quicker, short-term types associated with stationary time series. In contrast, we use a more general model that allows for a range of decay patterns.[40] Our use of the more general model allows us to *estimate*, rather than to assume, the degree of persistence in aggregate levels of partisanship.[41]

What are the implications of aggregate partisan behavior that is persistent over a period of years but not months or decades? As one example of the consequences of our conclusion, consider Carmines and Stimson's (1989) work on issue evolution in the U.S. electorate. As they argue, the temporal path of an issue evolution is intimately intertwined with the dynamics of aggregate partisanship. If issue evolution does indeed depend on the dynamics of partisanship, as Carmines and Stimson suggest, then our empirical evidence implies that the complete evolution of a partisan cleavage on a particular issue could be accomplished by the party of a two-term president and need not take "a very long time to complete" (Carmines and Stimson 1989, 193). Thus, our evidence that shocks to partisanship persist for years, in conjunction with our look at potential micro-level assumptions accounting for these aggregate patterns of growth or decay, provides a conceptually richer and empirically more precise basis for the concept of issue evolution.

Similarly, our empirical evidence that shocks to aggregate partisanship persist for years, but not months or decades, is consistent with Gerber and Jackson's (1993) evidence about endogenous preferences. They found changing relationships between partisanship and preferences over an eight-year period (for civil rights) and a four-year period (for the Vietnam War). We, too, find evidence of a years' long dynamic within the macro-level party system between 1953 and 1992.

In addition, our work has implications for controversies about the effects of question wording on conclusions about the degree of persistence in aggregate partisanship (Abramson and Ostrom 1991, 1992, 1994; Bishop, Tuchfarber, and Smith 1994; MacKuen, Erikson, and Stimson 1992). Using aggregate measures of party identification based on Gallup surveys, we find a high degree of persistence in aggregate partisanship. Thus, our results disconfirm the hypothesis that aggregate partisanship as measured by Gallup surveys will exhibit high volatility in response to shocks.

In our analysis, we attempt to bridge the gap between individual- and aggregate-level partisan behavior. In doing so, we have obtained important insights into the degree of persistence in macro-level partisanship. What we now know is that there is a medium-term dynamic of years—not months or decades—within the party system. We also know that this dynamic is potentially caused by one of three factors: behavioral heterogeneity in the electorate, persistence in exogenous political factors that influence individual party identification, or the interaction of heterogeneity and persistence. In future research, we plan to investigate these issues in greater detail by undertaking analyses of individual-level partisan behavior.

## APPENDIX

In this appendix, we repeat Granger's (1980) proof for aggregation under the conditions associated with case 3, in which behavior is not homogeneous and the degree of persistence is incomplete. Assuming $\delta = 0$, the individual decision mechanisms is given by:

$$x_{jt} = \alpha_j x_{jt-1} + \varepsilon_{jt}, \qquad \textbf{(A-1)}$$

where $x_{jt}$ is the $j$th individual's current party identification, $x_{jt-1}$ is the last period's partisan affiliation for the $j$th individual, and $\varepsilon_{jt}$ is a mean zero, white noise process. (We use $j$ as the subscript for individuals because we later use $i$ to denote complex numbers.) According to Granger, when N is large and the $\alpha_i$ are drawn from a beta distribution on the range (0, 1) with $p, q > 0$, then aggregating over the behavior shown in A-1 yields an aggregate time series that is fractionally integrated.

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[39] The term "mid-range dynamics" is due to MacKuen, Erikson, and Stimson (1989). We remind the reader that our criticisms of MacKuen, Erikson, and Stimson's (1989) work apply only to their conclusions about the *magnitude* of aggregate persistence.

[40] It appears that Nardulli (1995, 10), who finds evidence that critical realignments are regional phenomena that "endure for decades," imposes a unit root in his analyses. This restriction would constrain him to find higher degrees of persistence than those associated with fractionally integrated processes.

[41] It is possible to approximate a fractionally integrated process using an autoregressive model with a *large* number of AR($p$) parameters (Granger 1980, 231). MacKuen, Erikson, and Stimson (1989), however, use only an AR(1) transfer function to model macropartisanship. They thus omit a large number of lagged AR($p$) terms, which seriously biases their empirical estimates of the effects of shocks to macropartisanship.

Granger's (1980) proof proceeds as follows. First, consider the power spectrum of the aggregate series:

$$\bar{f}(\omega) = \sum_{j=1}^{N} f_j(\omega),$$

where

$$f_j(\omega) = \frac{1}{|1 - \alpha_j z|^2} \cdot \frac{var(\varepsilon_{jt})}{2\pi}, \text{ and } z = e^{-i\omega}. \tag{A-2}$$

Assuming that the $\alpha_j$ are random variables drawn from $F(\alpha)$ and assuming that the var $(\varepsilon_{jt})$ are random variables that are independent of the $\alpha_j$, Granger (1980, 231) states that the power spectrum of the aggregate series can be approximated as:

$$\bar{f}(\omega) \simeq \frac{N}{2\pi} E[var(\varepsilon_{j,t})] \cdot \int \frac{1}{|1 - \alpha z|^2} dF(\alpha). \tag{A-3}$$

After some manipulation of the denominator in the integral, Granger (1980, 232) shows that when $F(\alpha)$ is given by the beta distribution, then equation A-3 can be reexpressed so that the coefficient of $z^k$ in the power spectrum is

$$\frac{2}{B(p, q)} \int_0^1 \alpha^{2p+k-1}(1 - \alpha^2)^{q-2} d\alpha. \tag{A-4}$$

Granger (1980, 232) notes that this coefficient equals $\bar{\mu}_k$, the $k$th autocovariance of the aggregate series, which for large $k$ (and with $q >$ 1) can be approximated by

$$\bar{\mu}_k = A_1 k^{1-q}. \tag{A-5}$$

Granger (1980) notes that the differential equation in A-5 has the same form as the differential equation that defines a fractionally integrated process:

$$\rho_j \simeq A_1 j^{2d-1}. \tag{A-6}$$

Thus, equating coefficients, we see that $d = 1 - q/2$, and the aggregate series is fractionally integrated.

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# Strategic Policy Considerations and Voting Fluidity on the Burger Court

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*Justices are strategic actors. This is particularly evident when they change their votes between the original conference on the merits and the Court's announcement of the final decision. We predict that such voting fluidity may be influenced by strategic policy considerations, justices' uncertainty over issues involved in a case, the chief justice's interest in protecting his prerogatives, and other institutional pressures. To test our hypotheses, we explore the occurrence of fluidity on the Burger Court (1969–85). Using logistic regression, we show that justices' decisions to change their votes stem primarily from strategic policy considerations. In limited instances, the decision to switch can be attributed to either uncertainty or institutional pressures. Our findings suggest that the decision of a justice to join an opinion results from more than his or her initial policy preferences; final votes are influenced as well by the politics of opinion writing.*

> Justices, unlike computers, do not react automatically or always "stay put"....
>
> —Chief Justice Warren Burger, letter to Justice William O. Douglas, July 27, 1972

As Chief Justice Burger suggests, Supreme Court justices change their votes. They may reconsider the vote they cast at the initial conference on the merits at any time before the Court's final decision is announced. Justice William Brennan, for example, once told Justice Potter Stewart, "I voted the other way at conference but you've convinced me" (Brennan 1969). What prompts a justice to change positions? Murphy (1964) suggests that the answer rests, at least in part, in the strategic interaction among justices. After all, the key resource that policy-minded justices have at their disposal is their vote and opinion—and these can be used as bargaining chips to affect the content of the Court's opinion. It thus seems likely that justices might switch their votes in response to draft opinions as they attempt to obtain final Court opinions consistent with their views.

The nature of this strategic interaction is frequently seen in exchanges over opinion passages. Justices often ask the opinion author to address their concerns by modifying the draft opinion through the addition of a phrase or the deletion of a few sentences. These requests are frequently presented as a condition for joining the opinion. Justices tacitly threaten to withhold support if the changes are not made; indeed, they may refrain from joining the opinion and may instead write or join a concurring or dissenting opinion. At times, justices are more blunt. After asking Justice Hugo Black to make two additions to his majority opinion, Justice Potter Stewart threatened to join John Harlan's dissent: "At the risk of seeming unreasonably stubborn, I am still unwilling to join your opinion so long as it contains the view expressed in the phrase 'over a long period of time' in the 6th line on page 12. Perhaps I had better wait to see John Harlan's separate opinion" (Stewart 1969). Ultimately, Black altered the draft opinion, and Stewart switched from the position he first expressed at conference and joined Black's opinion.

Even though Chief Justice Burger and most judicial scholars recognize that justices alter their votes, our understanding of why and when they switch positions is still limited. Indeed, since the first discussion of justices' strategic behavior by Murphy (1964), very little empirical work has been done to examine the nature of this strategic interaction (Epstein and Knight 1995). In this paper, we articulate and test a comprehensive model of why justices do not always "stay put." We argue that justices' decisions to alter their votes are potentially shaped by three factors: strategic policy considerations, uncertainty about the legal issues involved in a case, and institutional pressures and considerations inherent in the Court.

## VOTING FLUIDITY

Fluidity is not the norm on the Court, but neither is it rare: During the 1946–75 period, a justice was likely to switch his or her vote from affirm to reverse or from reverse to affirm 7.3% of the time (Dorff and Brenner 1992).[1] Nevertheless, over half of Supreme Court cases decided during the six terms covered in Brenner (1980) had at least one instance of fluidity. Following the landmark work by Howard (1968), several studies have attempted to examine and explain empirically the occurrence of fluidity (Brenner 1980, 1982; Dorff and Brenner 1992; Hagle and Spaeth 1991). The explanations these authors offer have focused primarily on the attributes of individual justices and case characteristics, such as a justice's freshman status, the size of the majority coalition, and the salience of the case. We build upon previous analyses of voting fluidity in three respects.

First, we examine the relationship between strategic policy considerations and changing votes. In spite of the

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[1] During the 1946–55 period, a justice changed his vote from affirm to reverse (or vice versa) 9% of the time (Brenner 1980), and during the 1956–67 period, 10% of the time (Brenner 1982).

insight that voting fluidity provides judicial scholars in investigating the strategic interaction among justices, most studies do not explore the role of policy considerations. The only study that has tested that role (Hagle and Spaeth 1991) finds a significant bivariate relationship between the occurrence of fluidity and the juxtaposition of the author's and the voting justice's ideology. Other studies investigate other ideologically related hypotheses, such as whether fluidity is less prevalent among outcome-oriented, liberal justices or in cases with liberal outcomes (Dorff and Brenner 1992), but they do not address whether justices change positions because they are ideologically closer to the opposing writer than to the writer assigned the opinion for their original positions.

Second, with a single exception (Dorff and Brenner 1992), previous analyses of fluidity have not attempted to test a multivariate model. Instead, they have used bivariate analysis to isolate factors related to the frequency of voting fluidity, such as justices' length of experience on the Court. By examining the effect of one factor at a time, most previous work has not disentangled the effects of the multiple influences on fluidity. Multivariate controls allow us to assess the independent influence of numerous factors potentially affecting justices' votes. Conclusions drawn from previous bivariate analyses may not hold when multivariate controls are in place.

Third, we expand the scope of previous research to include all cases decided during the Burger Court (1969–85) and all instances in which a justice voted on the initial decision either to affirm or reverse.[2] Previous studies have tended to focus solely on either the tendency of justices to switch into the minority or on their propensity to join the majority. Although Brenner (1980, 1982) did assess the frequency of both types of fluidity, no study has yet developed a comprehensive model of fluidity. For example, although Dorff and Brenner (1992) use a multivariate model, their analysis is based on a limited data set assessing only the votes of justices who initially sided with the minority. In contrast, Hagle and Spaeth (1991) limit their analysis to justices originally voting with the majority; and Brenner, Hagle, and Spaeth (1989) look solely at fluidity that changes the outcome of a decision. All of these studies present important instances of fluidity, but none captures the complete range of fluidity that can occur. Collectively, therefore, they provide only a partial account of the politics of Court decision making. By building on these earlier important works, we hope to develop a more complete model of the politics of fluidity and to expand our understanding of Court dynamics.

## FLUIDITY AND THE DELIBERATIVE PROCESS

Voting fluidity on the Supreme Court presents a paradox to judicial scholars. Although Chief Justice Burger asserts that justices do not act "automatically," the dominant explanations of judicial behavior virtually lead us to expect justices to do so. At the very least, vote switching seems inconsistent with dominant explanations of Court behavior (Howard 1968). The attitudinal model of Court decision making suggests that preferences alone account for the behavior of Supreme Court justices (Segal and Spaeth 1993, 65).[3] Indeed, attitudinalists view justices as " 'naive' decision makers who always vote their unconstrained attitudes" (Epstein and Knight 1995, 2). They argue that justices make decisions exclusively on the basis of their fixed preference concerning the case outcome, and the Court's written opinions are therefore just "post-hoc rationalizations" of their earlier decision on the merits, cloaking their true motivations (Frank [1930] 1970, Schubert 1965, Segal and Spaeth 1993). If this is an accurate depiction of judicial decision making, reevaluation of a justice's position and subsequent vote change as a result of the Court's deliberation and circulation of draft opinions is an anomaly. If justices have a clear set of policy preferences or attitudes that they apply in reaching the decision on the merits, what explains vote change?

In this article, we suggest a more nuanced role for the influence of policy preferences on justices' votes. In particular, we suggest that justices' votes may change while they are considering a case because of the context of the decision-making process. At least three elements of that process are likely to convince justices to alter previously held positions. First, their preferences are not always firm on every issue. Such uncertainty leaves them open to the persuasion of their fellow justices during the decision-making process. Second, the dynamics of the coalition-building process—in which opinions are drafted and amended—can often entirely reshape the nature of the Court's ruling and reasoning, thereby making the outcome more or less attractive to the justices. Third, institutional considerations may encourage justices to change their votes for reasons that are independent of their policy concerns. We elaborate each of these competing influences below.

Consider first the effects of justices' uncertainty about the legal and political issues raised in a new case. Justices may not have strong policy preferences about every issue that comes before the Court, and the policy implications of the case may not be apparent until the majority opinion is drafted. As a result, we might expect a justice who does not have fixed views on a given issue to switch his or her vote during deliberation of the case, perhaps after being persuaded by a fellow justice

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[2] Previous studies of Court fluidity have focused on fluidity during the Vinson and Warren Courts and the early years of the Burger Court. The opportunity to study fluidity during the entire Burger Court has only recently been provided by the release of Justice Thurgood Marshall's and Justice William Brennan's papers.

[3] While the attitudinal model may be the most prominent model of Supreme Court decision making, other scholars have maintained that preferences alone do not account for the actions of Supreme Court justices. For example, the decision to grant certiorari depends in part upon non–attitudinal factors, such as the interests of the solicitor general (Tanenhaus et al. 1963) or the need to correct errors made by lower courts (Provine 1980, Songer 1979, Ulmer 1984). Likewise, the chief justice's assignment of cases depends in part upon organizational demands upon the Court (Maltzman and Wahlbeck 1996a). Court decisions on the merits are also influenced by legal precedent (George and Epstein 1992), party resources (Songer and Sheehan 1992; but see Sheehan, Mishler, and Songer 1992), and attorney experience (McGuire 1995).

(Howard 1968, Murphy 1964). Murphy explains that "judges can be persuaded to change their minds about specific cases as well as about broad public policies, and intellectual persuasion can play an important role in such shifts" (1964, 44). The importance of persuasion is reflected in a memo from Justice Harlan to Justice Black about his decision to change his vote in *Detroit and Toledo Shore Line* v. *United Transportation Union* (1969). Harlan wrote Black, "As one of those whose Conference vote was to reverse this case, I am persuaded by your opinion that affirmance is the proper result" (Harlan 1969). Preferences, in short, may be endogenous to the decision-making process. Rather than acting on sincerely held preferences brought to the bench, justices' views about particular cases may be susceptible to influence during the process of deliberation.

Although a decision to change a vote may reflect an evolution of thinking after the initial conference vote, we suspect that an important source of fluidity may be the nature of the opinion drafts that are circulated. Justices and members of the legal community care about more than the decision to reverse or affirm: they care about the legal rules articulated in the opinions. After all, these legal rules not only affect the litigants in a specific case, but also establish guidelines or referents that will affect similarly situated parties (Spriggs 1996, Wahlbeck 1994). The Court's opinions can therefore be seen as broad policy statements. What these opinions say, of course, depends upon both the policy views of the opinion author and the composition of the coalition expected to sign the opinion. Thus, fluidity in a justice's vote is likely to reflect a response to the opinion circulated after the conference on the merits.

Take, for example, *Wardius* v. *Oregon* (1973). In this case, Justice Brennan's docket books reveal that Justice Rehnquist initially voted with the minority. Nevertheless, on March 14, 1973, he circulated an explanatory note: "I voted to affirm in this case at Conference, but before writing a dissent to Thurgood's proposed opinion I think I will wait to see if anything narrower . . . is written" (Rehnquist 1973). On June 4, Justice Marshall circulated a new draft with a cover letter stating that "since the Court appears hopelessly splintered on the disposition of petitioner's contentions concerning the state's exclusionary rule, I have decided that it may be best to leave this question for another day" (Marshall 1973). On June 6, Marshall's decision to limit the breadth of his opinion was rewarded when Rehnquist joined his opinion to reverse. Rehnquist's vote in this case clearly reflected something other than a desire to affirm the decision of the lower court. Instead, Rehnquist was willing to abandon his preferred outcome in order to ensure that the majority opinion was narrowly drawn. Thus, Rehnquist changed his vote once the opinion as crafted by Marshall more closely reflected his preferred policy outcome. Changes in draft opinions may yield changes in justices' votes.

Beyond such strategic interactions among opinion authors and their coalitions, institutional considerations might also be at play in leading justices to change their votes. As members of a nine-justice bench, Supreme Court justices are potentially susceptible to a variety of pressures beyond policy preferences. First, the visibility of each justice's vote exerts unique pressures on the Court. Justices on the Court, like individuals in all institutions, may feel uncomfortable remaining outside the dominant group. As a result, justices may change their votes from the minority to the majority because of small-group influences that are independent of policy preferences. Second, justices may feel constrained by concerns about the effects of the Court's decisions. This constraint is potentially enhanced by producing a unanimous opinion from the bench (Johnson and Canon 1984).

In sum, preferences alone—independent of the process of reaching decisions on the Court—cannot explain why justices change their votes. Indeed, according to Howard (1968), such voting fluidity suggests the need to refine the attitudinal model: "If a vote or an opinion has changed in response to a multiplicity of intracourt influences before its public exposure, how reliable is that vote or opinion as an indicator of attitude, ideology, or if one pleases, predilection?" (p. 44). In our view, a model of voting fluidity must take into account both policy preferences and these "intracourt influences."

## EXPLAINING FLUIDITY

Fluidity stems from the strategic interaction among justices. We therefore expect justices to switch to the opinion that best represents their policy preferences and we expect this relationship to hold even after subjecting it to controls for issue uncertainty, the chief justice's interest in leading the Court, and other institutional considerations. We address each of these in turn.

### Policy Considerations

The strategic model portrays justices as responding to the positions articulated by other justices. As a result, we expect justices to sign the opinion that is closest to their own policy preferences (Epstein and Mershon 1993, Rohde and Spaeth 1976, Segal and Spaeth 1993). Previous findings on voting fluidity are consistent with this expectation. For example, Hagle and Spaeth (1991) show that nearly 75% of the time when a justice switches from the majority to the minority, s/he is ideologically closer to the marginal justice in dissent than to any member who sides with the majority (p. 122). Similarly, Brenner and Spaeth (1989) and Brenner, Hagle, and Spaeth (1989) show that minimum winning coalitions are most likely to break up when the marginal justice in the majority is closer to the dissenters than to any member of the majority. In these instances, the marginal justice is likely to switch and undermine the initial majority's victory.

The extent to which a particular opinion reflects a justice's policy preferences depends upon three factors. First, the views of the authors writing majority and dissenting opinions are important. After all, as Justice William Brennan once wrote to Chief Justice Burger, "[My] votes are tentative depending upon what is written pro and con" (Brennan 1971). Because the justices who write opinions have a disproportionate capacity to shape the final opinion (Murphy 1964), the following hypothesis seems reasonable:

Hypothesis 1: *The closer a justice is ideologically to the person writing the opinion for his or her conference coalition as compared to the author of the opposing opinion, the less likely the justice is to switch.*

Second, the ideological composition of the original coalition may influence a justice's final vote on a case. Although authors clearly have a significant influence over a particular opinion, the justices who make up a coalition can also substantively affect the opinion they expect to sign (Epstein and Knight 1995, Murphy 1964, Schwartz 1996). Authoring justices frequently respond to the concerns articulated by other potential coalition members by adopting their suggestions. Justice Stewart, for example, once reported to the conference that he "restructured this opinion somewhat, in an effort to meet the difficulties expressed" by other justices (Stewart 1970). Thus, as justices attempt to retain a majority, the views they articulate in an opinion may be moderated by a desire to appease another justice. This leads us to expect the following:

Hypothesis 2: *The closer a justice is ideologically to the original coalition as compared to the opposing coalition, the less likely the justice is to switch.*

Third, the size of the initial coalitions is likely to be important. If the initial majority coalition is a minimum winning coalition, authors on both sides will recognize the fragility of their coalitions and thus be particularly responsive to the concerns of those justices forming the original coalition. Until the opinion commands a "Court" of five justices, the author has an incentive to accommodate other justices to reach a majority (Biskupic 1995, Riker 1962). This leads to the following hypothesis:

Hypothesis 3: *A justice is less likely to switch votes if the initial majority coalition was a minimum winning one.*

## Uncertainty

There are, of course, alternative explanations to account for a justice's decision to switch his or her vote. A plausible hypothesis is that the justice did not fully understand the issues involved in a particular case when casting the initial vote. Indeed, in his ground-breaking study of fluidity on the Court, Howard (1968) suggests uncertainty as a factor that could account for such voting fluidity: "Shifting perspectives appear to have been a function of additional thought and homework, by a clerk or a justice, into issues that were only partially perceived at first because of inadequate argument, briefs, or time" (p. 47). This is supported by statements of justices indicating how they reached results contrary to their original positions—"mature reflection" or "research and labor" convinced them to change votes (Harlan 1970, Marshall 1969).

A justice's uncertainty should vary with both the case and the justice. When a case is particularly complex and thus raises multiple legal issues, the likelihood that a justice will understand all aspects of the case at the time of the initial vote is low. Justice William Douglas suggested this to the conference in *Dandridge* v. *Williams* (1969):

> I voted to reverse. As you will recall, the decision by the three-judge court was on the Equal Protection Clause that I think did not sustain the Court's position.... I have now concluded that the case was rightly decided below, not for the constitutional reason but because of the federal statute and the Supremacy Clause. I am, therefore, changing my vote to affirm (Douglas 1969).

Thus, we hypothesize the following:

Hypothesis 4: *The more complex the case, the more likely a justice is to switch.*

Conversely, a justice's level of certainty will be highest when the case is highly salient. In these cases, the justice's positions will be relatively fixed, and the likelihood that new information will alter a justice's opinion is relatively low (Brenner and Palmer 1988). Consistent with this finding are Brenner's (1980, 1982) and Hagle and Spaeth's (1991) findings that fluidity is not more likely in a case that is particularly salient. We expect little fluidity in highly salient cases, but justices may be more willing to acquiesce to a contrary opinion in cases that are not seen as important. In fact, Dorff and Brenner (1992) show that justices are more likely to switch from the minority to the majority if the case is not salient. This leads us to expect the following:

Hypothesis 5: *The more salient the case, the less likely a justice is to switch.*

Uncertainty is also likely to reflect a justice's judicial experiences and personal expertise. The longer a justice has served on the Court, the more likely he or she is to understand all of the relevant issues when casting the initial vote on merits. In contrast, "it is not uncommon for a new justice to undergo a period of adjustment ... before his voting behavior stabilizes into observable, not to mention predictable, patterns" (Howard 1968, 45). Consistent with this claim, Dorff and Brenner (1992) find that freshmen justices are more likely to switch than are their senior colleagues (but see Hagle and Spaeth 1991). Conversely, a justice's relative expertise in an issue area will also likely affect the propensity to switch. Presumably, justices with substantive expertise in a particular area are more likely to cast an informed vote at the initial conference than are their colleagues who have only a passing familiarity with a particular type of case. These expectations lead to two further hypotheses:

Hypothesis 6: *Freshmen are more likely to switch than are more senior justices.*

Hypothesis 7: *Policy experts are less likely to switch than are justices with less expertise in an issue area.*

## Institutional Considerations

The chief justice is in a unique institutional position. Unlike other justices on the Court, the chief justice has the prerogative to assign the Court's opinion when s/he is in the majority. This prerogative is one of the chief's most important sources of power (Maltzman and Wahlbeck 1996a, Murphy 1964, Rohde 1972, Rohde and Spaeth 1976, Segal and Spaeth 1993, Slotnick 1979). For this reason, it may be in the chief justice's strategic interest to switch from the minority to the majority after conference. Although judicial scholars have not studied the frequency of such strategic behavior, anecdotal evidence from the justices' papers and chronicles of Court politics have suggested that Chief Justice Burger frequently engaged in such behavior (Woodward and Armstrong 1979). For example, in an unpublished opinion, Justice Douglas accused Burger of trying to control the opinion assignment by altering his vote in *Roe* v. *Wade*:

> The Chief Justice represented the minority view in the Conference and forcefully urged his viewpoint on the issue. . . . Hence traditionally the senior Justice in the majority—who in this case was not myself—should have made the assignment of the opinion . . . . The cases were, however, assigned by the Chief Justice (Douglas 1972a).[4]

In addition to changing his or her vote to control the assignment, the chief may feel additional institutional pressures to vote with the majority. As the leader of the Court, s/he may seek to present the Court as unified and himself or herself as the leader. As a result, we might expect the chief to subordinate his or her policy preferences and switch to side with the majority. Regardless of why the chief moves from the minority to the majority, there is good reason to propose the following:

HYPOTHESIS 8a: *The chief justice is more likely than other members to switch from the minority to the majority.*

Conversely, the chief justice will want to retain control of the opinion and will rarely switch from the majority to the minority:

HYPOTHESIS 8b: *The chief justice is less likely than other justices to switch from the majority to the minority.*

The power of the chief to assign the majority opinion is not the only institutional feature of the Court. For two additional reasons, the nature of the initial voting coalitions might increase the probability that justices will switch their votes. First, Supreme Court scholars have maintained that the impact of Court decisions is enhanced when the Court rules overwhelmingly, sending a clear signal to lower court judges and other implementers (Howard 1968, Johnson and Canon 1984, Murphy 1964). As a result, Howard (1968) argues that minority coalition justices frequently alter their votes because of their loyalty to the Court as an institution. The size of the voting coalitions may affect justices' desire to change their votes for unconscious reasons as well. Indeed, social psychologists have maintained that an individual's perception of the appropriate conclusion is most likely to change when he or she is one of few dissenters (Asch 1951, 1952; Hare 1976). The influence of the majority is particularly intense in small groups (Hare 1976). Brenner and Dorff (1992) argue that this is why justices are more likely to switch from dissenting to concurring than from concurring to dissenting. This leads to a final set of hypotheses consistent with these expectations:

HYPOTHESIS 9a: *Justices who vote in the conference minority are more likely to switch than justices who are members of the conference majority.*

HYPOTHESIS 9b: *Justices who dissent are more likely to switch to the majority opinion if relatively few justices are dissenting.*

HYPOTHESIS 9c: *If the Court is unanimous on the initial conference vote, justices will be less likely to switch.*

## DATA AND METHODS

Our analysis is based upon cases decided by the Burger Court (1969–85 October terms).[5] By comparing the initial conference vote of each justice to his final vote, we were able to identify instances of fluidity. To determine the conference vote of each justice, we relied upon the docket books of Justice William Brennan. These docket books provide a highly reliable record of how each justice voted in conference, especially when the vote supported either affirmation or reversal (Maltzman and Wahlbeck 1996b). To determine the final vote of each justice, we used Spaeth (1994a).

If a justice voted to affirm at the initial conference on the merits and eventually supported reversal in the final decision or, alternatively, initially voted to reverse but ultimately affirmed the lower court, we coded the justice's action as fluid.[6] If the justice voted either to affirm or reverse initially as well as in the final decision, we portrayed the justice's vote as stable. If a justice voted for an outcome other than to reverse or affirm (for example, to "dismiss as improvidently granted" or vacate) at either vote, the justice's pair of votes was excluded from the analysis. During the Burger Court, there were 18,411 pairs of individual votes. Of these, 2,899 (15.7%) were excluded from our analysis because of a vote for a position other than to affirm or reverse or because the majority supported such a position. Another 53 vote pairs were omitted because the conference vote was tied.[7] Because our dependent variable is dichotomous (identifying whether a justice changed his or her vote after the original conference on the merits), we used a logistic regression model (Aldrich and Nelson 1984, King 1989). The hypotheses suggest that a justice's decision to switch depends upon a series of justice- and case-specific variables.

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[4] Based on an examination of the justices' conference vote, some might question Justice Douglas's assertion that he was not the senior associate justice in *Roe*. In fact, at least according to his records, he was the most senior justice in the coalition that favored striking down the abortion laws. In a letter to Chief Justice Burger, he explained why he was not the senior associate justice: "The reason Bill Brennan, not I, represented the consensus at the first Conference on the Abortion Cases was that I thought at the time that the cases—at least Georgia's—could be disposed of on Equal Protection grounds—a theory that did not hold up on further study" (Douglas 1972b).

[5] We limited our analysis to cases that were orally argued before the Court and in which the Court released a signed opinion. We omitted per curiam decisions, memorandum decisions, and decrees. We also excluded those cases in which no clear majority existed at the initial conference because there was a tied decision or because a majority of the Court voted for an outcome other than to reverse or to affirm. The docket sheets for the 57 cases missing from Brennan's records are also excluded. Finally, we excluded those cases that did not fit into one of the 12 substantive issue areas that Spaeth (1994a) identified. (See footnote 8.)

[6] We limited our analysis to those instances in which a justice initially voted to affirm or reverse because in these instances the docket books of the justices are most reliable (Maltzman and Wahlbeck 1996b). If we included in our analysis votes that were neither to affirm nor to reverse, what would appear to be fluidity could indeed simply reflect docket-book coding errors.

Next, we describe how we constructed the independent variables necessary to test the hypotheses of voting fluidity.

## Justice-Specific Attributes

***Author Distance.*** To assess the ideological distance between a justice and the authors of the majority and minority opinions, we calculated an issue-specific ideological score for the compatibility between each justice and both the majority opinion author and the dissent author closest to the justice in question. To determine this score, we used twelve substantive value groups identified by Spaeth (1994a).[8] For each of these groups, we calculated the percentage of cases in which each justice voted for the liberal outcome (Epstein et al. 1994, Table 6-1). We calculated author distance by subtracting the difference between each justice and the author on the other side of the case from the distance between each justice and the author on the side the justice initially supported.[9] When a justice was closer to the opposing writer than to the justice writing the opinion for the side a justice initially supported, our measure produces a positive value. If the writers were equidistant from a justice, the author distance is zero. If a justice was closer to the writer for the side s/he initially supported than to the opposition writer, the measure is negative. Hypothesis 1 leads us to expect fluidity to be positively related to author distance.

***Coalition Placement.*** To determine a justice's position in relation to the coalitions that formed at the initial conference, we compared each justice's issue-based ideological rank with the mean rank for both the minority and majority coalition that formed at this conference.[10] We gave a justice a placement score of +1 if his or her ideological rank was either at the initial coalition's mean or was located between that mean and the rank most closely associated with the outcome supported by the conference coalition, that is, one for liberal outcomes and nine for conservative outcomes.[11] If a justice's rank was located between the mean member of the two coalitions that initially formed, the justice's position was coded as 0. If a justice's rank was located between the opposition coalition mean and the rank identified with the outcome supported by that coalition, we coded the justice's placement as −1.[12] Hypothesis 2 leads us to expect a negative correlation between fluidity and coalition placement.

***Freshman.*** Any justice who had served less than two full years when the oral argument was heard was coded as 1. All others were coded as 0.[13] Hypothesis 6 leads us to expect a positive correlation between freshman status and fluidity.

***Expertise.*** To measure whether a justice had substantive policy expertise, we calculated an issue-specific opinion ratio (OR) for each justice.[14] The OR is based on the total number of majority opinions, dissents, and concurrences written by each justice during his or her entire tenure on the Supreme Court.[15] More specifically, the OR is the number of cases in which a justice wrote an opinion, dissent, or concurrence, divided by the number of like cases that had reached the Court since that justice's appointment. Since the OR takes into account all opinions written by each justice over his or her Supreme Court career, it is insensitive both to a justice's ideological position relative to colleagues and to the chief justice's assignment decisions. If a justice's OR was more than 1.65 standard deviations above the mean OR for the justices serving when a case was heard, we considered the justice a specialist in that issue area.[16] If a justice was a specialist in an issue area relevant to a particular case up for assignment, that justice was coded as 1. Others were coded as 0. Hypothesis 7 leads us to expect a negative relationship between expertise and fluidity.

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[7] Although we excluded votes that were neither to affirm nor to reverse from our dependent variable, several of our independent variables are based upon the size and makeup of the coalitions that formed on the initial merits vote, i.e., minimum winning coalitions. Excluding votes other than affirm and reverse from the calculation of those independent variables would obviously create a tremendous amount of measurement error. A justice's vote to dismiss a case does not mean that the other justices were unaware of how this justice would have voted if the Court had opted to decide the case. Justices frequently voice more than one preference at the conference. A justice might say, for example, that s/he favors dismissal, but would also support an affirmation of the lower court decision. While we could have omitted the entire case if this problem arose, this would raise questions about the generalizability of the findings. In 492 cases during the Burger Court, a justice voted for an outcome that was neither to reverse nor to affirm. To keep these cases in our analysis, we opted to substitute a justice's final vote for any initial vote that was for an outcome other than to reverse or to affirm.

[8] Spaeth (1994a) identifies 13 value groups. His 13th group consists of miscellaneous cases. Because of the ambiguous nature of this value area, we dropped these cases from our analysis.

[9] If a case was unanimously decided, no justice served as a referent for the opposition author. To solve this problem, we created an opposition author referent score. This score is the polar opposite of the outcome that was reached. If the Court unanimously voted for a conservative outcome, the opposition author ideology score was treated as 100% liberal. Conversely, if the Court voted unanimously for a liberal outcome, the opposition-author ideology score was treated as 0% liberal. This method is similar to Hagle and Spaeth's (1991, 122) treatment of unanimous cases.

[10] This procedure is analogous to a Wilcoxon rank-sum test that determines whether the median of two groups is similar. We used it to develop an indication of the coalition's ideological composition. On the basis of each justice's liberal support score, we assigned a rank of 1 to 9 to each justice on each of the 12 issue areas (Spaeth 1994a). We then summed the ranks of justices in each coalition and divided by the number of justices.

[11] We derived the ideological direction of the case outcome from Spaeth (1994a).

[12] If a case was unanimously decided, we created an opposition coalition referent score. This score is the polar opposite of the opinion that was reached. If the Court unanimously voted for a liberal outcome at its initial vote, justices who were more liberal than the mean of all justices were given a +1 placement score. Justices who were more conservative than the mean were given a 0 placement score. No justice was given a −1 score on a unanimous case. A similar coding scheme was employed for unanimous cases with a conservative outcome.

[13] To calculate each justice's length of tenure, we compared the oral argument date and the anniversary of the justice's Senate confirmation (Epstein et al. 1994, Table 4-12).

[14] Each justice was given a unique opinion ratio for each of 133 narrow issue areas that Spaeth (1994a) identifies. To select the 133 issues, we relied upon the 263 issue categories that Spaeth names and then grouped those issues that Spaeth reports as being related. For example, the five specific issue areas that Spaeth identifies as related to federal transportation regulation (railroad, boat, truck, pipeline, and airline) are grouped together.

[15] For justices Black and Douglas, we used the total number of opinions written since 1953 because this variable was generated from Spaeth (1994a), whose data begins in 1953.

***Chief Justice.*** To see whether the chief justice acts differently from other members of the Court, we created two additional dummy variables. The first is a dummy variable to designate the chief. The second variable designates the chief justice when s/he initially voted with the minority. Because the chief has a strategic interest in changing a minority vote to a majority vote, we call this variable *Strategic Chief.* Hypothesis 8a suggests a positive correlation between the chief's being in the minority (*Strategic Chief*) and switching his final vote. Hypothesis 8b suggests a negative correlation between the chief and fluidity in other circumstances.

***Minority Coalition Member.*** If a justice was a member of the minority coalition at the conference on the merits, this variable was coded as 1; otherwise it was coded as 0. Hypothesis 9a leads us to expect a positive relationship between conference minority voters and fluidity.

### Case-Specific Attributes

***Minimum Winning Coalition (MWC).*** If a case was decided at conference by a margin of one vote or less, it was coded as 1; otherwise it was coded as 0. Hypothesis 3 suggests that when a case is decided by a minimum winning coalition, justices will be less likely to switch.

***Case Complexity.*** We anticipate that a justice will be more likely to switch in cases that raise complex legal issues than in simpler cases. Although numerous measures of case complexity exist, no one measure fully captures the concept of complexity. Thus, we measured case complexity by factor analyzing three indicators and then used each case's factor scores as a measure of complexity. The joint variance, we argue, best captures case complexity. The first measure was the number of issues raised by a case.[17] The second was the number of legal provisions relevant to a case.[18] The third was the number of opinions released in a case.[19] Factor analysis of these three indicators produced a single factor with an eigenvalue of greater than 1. Since we presume that complex cases raise more issues, pertain to more legal provisions, and result in more opinions than do simpler cases, Hypothesis 4 leads us to expect our factor scores to be positively related to voting fluidity.

***Case Salience.*** Hypothesis 5 predicts that a justice will make a more informed decision at the initial conference if the case is highly salient. Thus, fluidity should be rare in salient cases. As an indicator of case salience, we used the Epstein et al. (1994, Table 2-10) list of landmark cases. If a case was included on the list, it was coded as 1; otherwise it was coded as 0.

***Dissent.*** To test the hypothesis that a justice who initially votes with the minority will be more likely to join the majority if relatively few justices initially voted to dissent (Hypothesis 9b), we created a dissent-size variable. If a justice was the only dissenter, this variable was 4. If two justices voted to dissent, the variable was 3; if three justices dissented it was 2; and if four justices dissented it was 1. We reversed the direction of our dissent count because all justices who voted with the majority were given a 0 for their dissent count. Our expectation is that our dissent variable will be positively correlated with a justice's decision to switch his or her vote.

***Unanimity.*** If every participating member of the Court initially voted with the majority, it was coded as 1; otherwise it was coded as 0. Hypothesis 9c suggests that a justice is unlikely to switch to the minority if the Court was initially unanimous. Thus, we expect a negative coefficient.

## FINDINGS

Table 1 provides an overview of our dependent variable. The table shows how frequently each of the 13 justices who served on the Burger Court changed his or her vote after the initial conference. During the Burger Court, at least one justice changed positions in 36.6% of the cases, while individual justices switched their votes 7.5% of the time. The table also shows that there was considerable variation by type of vote and across members. Whereas the average justice who voted with the minority at the initial conference changed his or her vote 18.1% of the time, a justice who initially voted with the majority was likely to switch 4.6% of the time. Justice Black switched 14.9% of the time and Justice Marshall 10.7%, but Justice Stevens switched only 5.8% of the time.

Table 2 presents the estimated coefficients and significance levels for a maximum likelihood logit model

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[16] The decision to use 1.65 standard deviations should result in a 5% likelihood that a justice will specialize in an issue area. During the natural court that began with Sandra Day O'Connor's appointment, for example, 31 of the 133 issue areas (23.3%) had no specialist, while 71 areas (53.4%) had a solitary specialist. In 28 areas (21.1%), two justices were specialists, and three areas (23%) had three specialists.

[17] We relied here upon Spaeth's (1994a) case classification system. Spaeth classifies each case by 263 issues. The variable we employed is a count of the number of issues that Spaeth identifies for each case. During the Burger Court, this count ranged from one to three.

[18] This number is a count of the legal provision variable in the Spaeth (1994a) data set. For the cases included in our analysis, the count ranges from one to six.

[19] The number of opinions was derived from Spaeth (1994a). Specifically, we counted for each case the number of justices who wrote a majority, concurring, or dissenting opinion. If a joint opinion, joint dissent, or joint concurrence was written, we assume that only one additional opinion, dissent, or concurrence was written. The maximum number of opinions that any case had during the Burger Court was seven.

**TABLE 1. Frequency of Vote Changes (Fluidity), by Conference Status and Justice**

| | Percent Fluidity When in | | |
|---|---|---|---|
| Justice | Conference Majority | Conference Minority | Overall Percent Fluidity |
| Black | 11.5% | 24.4% | 14.9% |
| Blackmun | 4.0 | 20.5 | 6.7 |
| Brennan | 5.1 | 15.7 | 8.5 |
| Burger | 4.3 | 17.8 | 6.3 |
| Douglas | 8.2 | 11.4 | 9.6 |
| Harlan | 5.4 | 27.3 | 8.6 |
| Marshall | 7.6 | 18.2 | 10.7 |
| O'Connor | 5.6 | 13.6 | 6.8 |
| Powell | 3.9 | 20.5 | 6.2 |
| Rehnquist | 3.3 | 23.4 | 8.9 |
| Stevens | 4.4 | 10.4 | 5.8 |
| Stewart | 3.8 | 18.2 | 6.5 |
| White | 3.3 | 22.5 | 6.2 |
| AVERAGE | 4.6 | 18.1 | 7.5 |

*Note*: This table shows how frequently each of the 13 justices who served on the Burger Court changed his or her vote after the initial conference. This is calculated by dividing the number of cases in which the justice changed votes by the justice's total number of conference votes in the majority coalition, the minority coalition, and overall.

**TABLE 2. Logistic Regression of Vote Fluidity (model 1)**

| Variable | Coefficient | Standard Error |
|---|---|---|
| Policy | | |
| Author distance (Hypothesis 1) | 0.061*** | 0.002 |
| Coalition placement (Hypothesis 2) | −0.459*** | 0.054 |
| Minimum winning coalition (Hypothesis 3) | −0.337** | 0.113 |
| Uncertainty | | |
| Case complexity (Hypothesis 4) | 0.112*** | 0.036 |
| Landmark (Hypothesis 5) | 0.071 | 0.155 |
| Freshman (Hypothesis 6) | 0.266* | 0.132 |
| Expert (Hypothesis 7) | −0.152 | 0.113 |
| Institutional | | |
| Chief Justice | −0.089 | 0.154 |
| Strategic Chief (Hypothesis 8) | 0.166 | 0.275 |
| Minority coalition member (Hypothesis 9a) | 0.913*** | 0.185 |
| Dissenters (Hypothesis 9b) | 0.382*** | 0.070 |
| Unanimity (Hypothesis 9c) | −0.143 | 0.130 |
| Constant | −3.030*** | 0.067 |
| Number of cases | 15,459 | |
| −2 × Log likelihood ratio | 5271.491*** | |
| Reduction of error | 24.700 | |
| Percent correctly classified | 94.300 | |
| Pseudo-$R^2$ | 36.200 | |

*significant at .05 (one-tailed test)
**significant at .01 (one-tailed test)
***significant at .001 (one-tailed test)

predicting whether a justice would switch votes. The statistically significant chi-square test of the log-likelihood ratio allows us to reject the null hypothesis that each of our independent variables, except the constant, is equal to 0. The pseudo-$R^2$ (36.2) demonstrates that the model explains the variance in fluidity moderately well. The model correctly classifies 94.3% (14,582) of the cases. Compared with predicting that a justice would never switch, the proportional reduction in error associated with the model is 24.7%.

We find strong support for the expectation that fluidity is a function of strategic policy calculations (Table 2). All three measures of a justice's ideological placement are statistically significant. The coefficient of the author-distance variable suggests that justices are more likely to switch if they are ideologically distant from the writer of the opinion to which they initially committed and ideologically proximate to the opposition writer. These findings provide empirical support for Hypothesis 1. The effect of the coalition placement score is also suggestive of the effects of policy preferences. The closer justices are to the opposition coalition, and the farther they are from their own coalition, the more likely they are to switch. This is consistent with Hypothesis 2. If the initial majority coalition is a minimum winning coalition, this also affects the likelihood of fluidity. Justices are less likely to switch if the original coalition was of minimum winning size. This suggests that in crafting opinions with the initial support of a minimum winning coalition, both majority and dissenting opinion writers are likely to attend to the preferences of their initial supporting coalition in order to hold it together.

To illustrate the substantive effect of the statistically significant independent variables reported in Table 2, we ran several simulations of the likelihood that a justice would switch. We held all significant independent variables constant except one, and the results in Table 3 show how each independent variable affects the likelihood that a justice would change his vote after the initial merits conference.[20]

The importance of such policy calculations is illus-

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[20] The benchmark probability is the likelihood of a vote change when the value of each variable is set at its mean or, in the case of a dichotomous variable, at the mode. By setting the values of our independent variables at various levels and then changing them one at a time, it is possible to observe the effect of each variable. For example, to simulate the probability of a justice changing his or her vote when s/he is closer to the writer of the opinion s/he initially supported than to the nearest justice writing on behalf of the other side ("closer to ally"), we set the author distance score at −96.8. This score would occur if a justice were perfectly aligned with the author s/he initially supported and if the nearest opposition writer were on the opposite end of the ideological spectrum. The simulation for the justice who is initially "closer to opponent" is calculated by reversing the placement of the authors. For the "closer to opponent" simulation, the author distance score is set at 85.5. The simulated probability of the "equidistant" justice is based upon an author distance score of 0. The simulated probabilities for coalition placement, number of dissenters, conference minority, and freshman justices are calculated for the categories specified in the table. For the simulated probability for the most complex cases, the complexity score is set at one standard deviation above the mean. Thus, the factor value for the "hard" complex case simulation is set at 0.97. This is compared to the "easy" case simulation where we set the complexity score at the minimum observed factor value of −0.98. This is about one standard deviation below the mean.

**TABLE 3. Simulated Probability of Vote Change**

| Characteristics | Probability |
|---|---|
| Benchmark | .018 |
| Author distance | |
| Closer to ally | .000 |
| Equidistant | .046 |
| Closer to opponent | .898 |
| Coalition placement | |
| Far side of initial coalition | .013 |
| Between coalitions | .021 |
| Far side of opposite coalition | .033 |
| Minimum winning coalition | .013 |
| Case complexity | |
| Easy | .016 |
| Hard | .020 |
| Freshman Justice | .023 |
| Dissenters | |
| 3 Justices | .077 |
| 1 Justice | .152 |
| Minority coalition member | .044 |

trated in these results. Whereas the justice who is equidistant between the authors switches 4.6% of the time, a justice with ideological views similar to those of the justice authoring the opinion within the original coalition is likely to change his or her vote 0.01% of the time. The likelihood of fluidity is 89.8% if the justice is ideologically proximate to the writer of the opposing opinion and distant from the writer authoring the opinion for the vote the justice initially cast. Similar patterns appear for the coalition placement score. Justices who are farther from the opposing coalition than from their initial coalition's mean ideological score are likely to switch 1.3% of the time. Conversely, justices who are on the far side of the opposing coalition's ideological mean are likely to switch 3.3% of the time. If the initial majority coalition is a minimum winning coalition, however, the probability of switching drops from the 1.8% benchmark to 1.3%.

In contrast, the model provides mixed support for the uncertainty hypotheses, no support for our expectation about the influence of the institutional factors unique to the chief justice, but strong support for our expectation about the general influence of institutional factors. Hypotheses 4–7 (predicting the effects of case complexity, case salience, justice experience, and justice expertise) suggest that varying degrees of uncertainty across cases and justices might affect voting fluidity. The results provide support for only two of these hypotheses. As indicated in Table 2, our measure of case complexity is significantly related to justices' decisions to switch votes. The likelihood that a justice will be fluid in a complex case is 25% greater than the likelihood of changing in an easy case. This outcome provides support for Hypothesis 4.

Contrary to Hypothesis 5, justices are not less likely to switch in salient cases.[21] We suspect that this is true because in salient cases opinion authors discourage fluidity by making an extra effort to accommodate each justice who is inclined to support their position. Epstein and Knight's (1995, 34) finding that in important cases opinion authors circulate more draft opinions is consistent with such an explanation. Consistent with Hypothesis 6, freshmen justices are significantly more likely to switch than are their more senior colleagues, as reflected in Table 2. Whereas the benchmark justice switched 1.8% of the time, the freshman justice switched 2.3% of the time (see Table 3). Contrary to Hypothesis 7, however, experts were just as likely as their colleagues to switch their votes. Although it is contrary to our hypothesis, this finding is understandable. Substantive experts are likely to be concerned with an opinion's content. Thus, experts may be just as willing as their less experienced colleagues to use their votes as leverage to encourage various opinion authors to write legal rules consistent with their preferences. This may lead them to change their votes even though their original votes were based upon a clear understanding of the law.

The statistical insignificance of the chief justice and strategic chief coefficients indicates that, contrary to Hypothesis 8, Chief Justice Burger did not act differently than did his colleagues. There is no evidence that Burger routinely changed his vote from the minority to the majority in order to control the opinion assignment. Although it is conceivable that the chief did switch his vote to control the assignment on a few salient cases, there is no general pattern to that effect.[22]

Institutional considerations also appear to have a powerful influence on justices decisions to switch votes. Justices who initially sided with the minority were more likely to switch their votes than those who sided with the majority (Hypothesis 9a). Compared to the benchmark, a justice was more than twice as likely to change positions when he or she was in the conference minority. The model supports Hypothesis 9b; for justices initially siding with the minority, the likelihood of joining the majority increased as the size of the minority coalition shrank. The simulated probabilities illustrate these dynamics. A justice who was one of three members of the minority had a 7.6% chance of switching. For a lone dissenter, however, the likelihood of switching more than doubled, to 15.2%. The negative but statistically insignificant unanimity coefficient in our model, however, does not support Hypothesis 9c.

Previous attempts to identify the causes of fluidity have focused on votes that altered the initial outcome, on justices who initially voted with the majority, or on justices who initially voted with the minority. Implicit in these studies is the idea that different sets of factors lead a justice to switch in different directions, thus requiring that directions of fluidity be modeled separately. For instance, if ideology led a justice to engage in conformity voting (changing a vote from the minority to the majority), and uncertainty led a justice to engage in counterconformity voting (changing a vote from the majority to the minority), the development of a general model would not make sense.

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[21] Nevertheless, some might suspect that salience explains fluidity only in combination with ideological distance. To test this, we reran the model so that salience interacted with both the writer and coalition placement variables. Both of these variables were insignificant, and all the variables that were significant in Table 1 retained their significance.

[22] One might argue that we have minimized the effect of the chief justice switching to the majority by exclusively examining conference votes to affirm or reverse. Conventional wisdom suggests that Burger, more frequently than any other justice, passed at conference. By not expressing a position at conference, Burger remained flexible and could join the majority and thus assign the majority opinion. According to Brennan's record of Burger's conference votes, he passed in 3.1% of the 2,065 cases in which he participated. This was the highest pass rate on the Court; Hugo Black's rate of 1.7% was the next highest. Burger's pass rate was significantly higher than those of all other justices taken together.

After we developed a general model of vote change that encompasses the full range of fluidity, we estimated a second model to test whether the influence of the factors we identified is conditional on the direction of fluidity (Table 4). This model includes all the variables used in the first model, which then interact with a dummy variable to signify whether a justice initially voted with the minority. The first model's variables interaction with a minority dummy variable enables us to test whether these variables have significantly different effects on conformity and counterconformity voting.[23]

The results suggest that the effects of most of the variables are not conditional on the direction of fluidity. Nevertheless, the model does suggest that there are significant differences between the processes for switching from majority and minority opinions. Two of the three policy variables (author distance and minimum winning coalition) are statistically significant and in the predicted direction. Members of the minority are even more likely than members of the majority to switch if they are ideologically closer to the opposing author than to the author of the opinion with which they initially sided. They are also slightly less likely to change their votes if there is a minimum winning coalition. In general, these results suggest that strategic policy considerations are magnified for justices who are in the minority, but the significance of uncertainty is diminished. This is demonstrated by the statistically significant and negative case complexity variable. We suspect that this results from the fact that justices who initially cast a minority vote may hold more intense convictions than the average majority justice and thus are less susceptible to the persuasion of their colleagues.

**TABLE 4. Logistic Regression of Vote Fluidity (model 2)**

| Variable | Coefficient | Standard Error |
|---|---|---|
| Policy | | |
| Author distance (Hypothesis 1) | 0.053*** | 0.002 |
| Coalition placement (Hypothesis 2) | −0.553*** | 0.070 |
| Minimum winning coalition (Hypothesis 3) | −0.189 | 0.132 |
| Uncertainty | | |
| Case complexity (Hypothesis 4) | 0.203*** | 0.042 |
| Landmark (Hypothesis 5) | 0.119 | 0.187 |
| Freshman (Hypothesis 6) | 0.344* | 0.165 |
| Expert (Hypothesis 7) | −0.102 | 0.148 |
| Institutional | | |
| Chief Justice | −0.056 | 0.151 |
| Strategic chief (Hypothesis 8) | 0.064 | 0.287 |
| Minority coalition member (Hypothesis 9a) | 1.324*** | 0.276 |
| Dissenters (Hypothesis 9b) | 0.225** | 0.095 |
| Unanimity (Hypothesis 9c) | −0.074 | 0.129 |
| Minority coalition | | |
| Author distance × minority coalition | 0.018*** | 0.004 |
| Coalition placement × minority coalition | 0.117 | 0.113 |
| MWC × minority coalition | −0.562* | 0.245 |
| Case complexity × minority coalition | −0.241** | 0.078 |
| Landmark × minority coalition | −0.069 | 0.332 |
| Freshman × minority coalition | −0.243 | 0.275 |
| Expert × minority coalition | −0.111 | 0.233 |
| Constant | −2.999*** | .072 |
| Number of cases | 15,459 | |
| −2 × Log likelihood ratio | 5,224.733*** | |
| Reduction of error | 26.500 | |
| Percent correctly classified | 94.500 | |
| Pseudo-$R^2$ | 36.700 | |

*significant at .05 (one-tailed test)
**significant at .01 (one-tailed test)
***significant at .001 (one-tailed test)

## CONCLUSION

Although fluidity on the Supreme Court is relatively rare, our findings suggest that it occurs in a predictable fashion. Whether joining the majority or defecting to the minority after an initial vote on the merits, the probability that a justice will switch is captured by a multivariate model of fluidity. While legal uncertainty might also make justices' initial conference votes more tenuous, our results suggest that voting fluidity is more likely to be conditional on policy and institutional considerations. Calculations about preferred policy outcomes and institutional motivations seem to lead justices to review their initial positions and occasionally to join the opposite coalition.

While strategic policy considerations affect justices in both the majority and minority, it is clear that these considerations play a more important role for justices who initially cast dissenting votes. A justice who initially dissented is more likely to change his or her vote if the conditions are ripe than is a justice in the majority. We suspect that the greater emphasis that minority justices place on these considerations stems from the fact that the majority opinion author has little incentive to be responsive to the suggestions of a dissenter. Since the majority opinion becomes the law of the land, justices who wish to shape the law have an incentive to be part of the majority. Consistent with this theory is the fact that justices who initially voted with the majority switched only 4.6% of the time, and justices who initially dissented switched 18.1% of the time (see Table 1).

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[23] Because our institutional variables (unanimity and number of dissenters) already take into account whether a justice is part of the minority at the initial merits conference, we are unable to repeat these two variables. The strategic chief variable also takes into consideration whether the chief initially voted with the minority. Thus, we do not repeat this variable in our second model.

These results shed light on the dynamic nature of coalition building within the Court. Although Murphy's (1964) classic study argues that decision making on the Court is a dynamic process in which justices bargain over preferred outcomes, judicial scholars often attribute judicial behavior to factors exogenous to the decision-making process. But our model suggests that fluidity is, at least in part, a response to the initial set of opinions and coalitions that emerges after the first conference. First, the content of the opinion and the position of its author are important: Justices who find themselves ideologically closer to the opposing coalition, for example, are more likely to switch than those who ideologies differ significantly. Second, the size of the initial coalition is also important. When defecting to the minority is likely to leave a justice isolated in the minority, s/he is less likely to defect; when joining the majority means escaping a small minority, however, a justice is likely to switch votes. Modeling fluidity to include switching both into and out of the majority thus gives us a broader perspective on the dynamics of coalition building on the Court.

Although this study makes clear that justices are willing to change their votes because of their policy preferences and their strategic calculations, it is important to note that changing a vote is probably the most drastic action a justice can take. Writing a concurrence or making a simple suggestion to an opinion author are strategic actions that are probably more common than is changing votes. Nevertheless, our understanding of these activities is based on only anecdotal evidence. Supreme Court scholars have made few attempts to study systematically the nonvoting activities in which justices engage. Of course, opinion authors themselves are also likely to act strategically. It seems reasonable to expect that under some conditions opinion authors would be willing to accommodate their colleagues, and in other instances they would be likely to use their position to implement their own goals. To develop a richer understanding of judicial behavior than the attitudinal model affords, a more thorough explanation of the coalition-building and opinion-writing processes is needed.

Over the past twenty years, scholars of the Court have articulated and refined an "attitudinal model" that has had a tremendous amount of explanatory power. As a result, few scholars have attempted to test empirically the model of strategic interaction that Murphy articulated in 1964. Recently, students of the Court have proposed building upon the attitudinal model by recognizing the kinds of strategic calculations that Murphy suggested dominate on the Court (Epstein and Knight 1995). Our findings provide strong support for moving the study of judicial politics beyond the attitudinal model.

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# "Race Coding" and White Opposition to Welfare

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*Crime and welfare are now widely viewed as "coded" issues that activate white Americans' negative views of blacks without explicitly raising the "race card." But does the desire of whites to combat crime or curtail welfare really stem from their dislike of blacks? Are these not pressing problems about which Americans rightly should be concerned—apart from any associations these issues may have with race? In this paper I assess the extent to which white Americans' opposition to welfare is rooted in their attitudes toward blacks. Using conventional survey modeling techniques and a randomized survey-based experiment from a national telephone survey, I find that racial attitudes are the single most important influence on whites' welfare views. I also show that whites hold similar views of comparably described black and white welfare mothers, but that negative views of black welfare mothers are more politically potent, generating greater opposition to welfare than comparable views of white welfare mothers.*

Political issues such as crime and welfare are now widely viewed as "coded" issues that play upon race (or, more specifically, upon white Americans' negative views of blacks) without explicitly raising the "race card" (Edsall and Edsall 1991, Jamieson 1992). Many believe that by engaging such issues, politicians can exploit whites' racial animosity and resentment while diminishing the appearance of race baiting. In the Willie Horton advertisement from the 1988 presidential campaign, for example, a pro-Bush political action committee used a black rapist and murderer who had been granted a weekend pass from a Massachusetts prison to symbolize Michael Dukakis's supposed softness on crime. Ostensibly, the focus of this ad was crime and what should be done about it; ineluctably, however, the subtext was race.

But does whites' desire to get tough on crime or their opposition to welfare really stem from their dislike of blacks? Are crime and welfare not pressing problems about which Americans rightly should be concerned, quite apart from any associations these issues may have with race? Welfare, crime, drugs, immigration policy, and other implicitly racial issues all have compelling claims on our attention irrespective of their racial connections, yet each is also undeniably shaped by race. Therefore, it is unlikely either that race plays no role in public thinking about these issues or that any of them represent simply racial politics in disguise. The question, then, is how important are racial considerations in the public's evaluation of race coded issues.

Surprisingly, we have little evidence to date with which to answer this question. Social scientists have produced a huge body of research on U.S. racial attitudes and their effect on explicitly racial policies, such as school busing, open housing laws, and affirmative action (e.g., Bobo 1983, 1988; McClendon 1985; Schuman and Bobo 1988; Sears 1988; Sears, Hensler, and Speer 1979; Sidanius, Devereux, and Pratto 1992; Sniderman and Piazza 1993; Sniderman, Piazza, Tetlock, and Kendrick 1991), as well as on broad political orientations, such as party identification (e.g., Carmines and Stimson 1989, Huckfeldt and Kohfeld 1989). But academics have thus far paid little attention to the influence of racial attitudes on support for ostensibly race-neutral policies dealing with issues such as crime and welfare.

In this article I show that whites' welfare attitudes are indeed strongly influenced by their views of blacks. Using conventional structural models of welfare views, I find that the perception that blacks are lazy has a larger effect on white Americans' welfare policy preferences than does economic self-interest, beliefs about individualism, or views about the poor in general. Then, using a randomized survey-based experiment with identical questions about whites' perceptions of black and white welfare recipients, I show that perceptions of poor blacks are more strongly related to overall evaluations of welfare than are perceptions of poor whites. Finally, I identify the precise source of the greater predictive power of perceptions of poor blacks, showing that neutral or positive views of black and white welfare mothers have similar influence on welfare attitudes, but that negative views of black welfare recipients generate greater opposition to welfare than do negative views of white welfare recipients. In sum, although elite debate is predominately cast in race-neutral language, the white public's thinking about welfare is inordinately shaped by highly salient negative perceptions of blacks.

## ATTITUDES TOWARD WELFARE

The U.S. welfare state contains a wide array of programs that supply a variety of benefits to diverse groups. Not surprisingly, surveys show dramatic differences in the public's evaluations of the various components of the welfare state. Education, health care, and benefits for the elderly receive nearly unanimous support among the U.S. public, as do programs that are seen as enhancing self-sufficiency (Cook 1979, Heclo 1986, Jaffe 1978). But support often turns to ambivalence or opposition when benefits are limited to the poor and provide direct cash or in-kind transfers. Programs such as Aid to Families with Dependent Children (AFDC), General Assistance,

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and Food Stamps, all of which offer assistance directly to poor families and individuals, are the least popular components of the U.S. welfare state (Shapiro and others 1987, Smith 1987).

These means-tested transfer programs, often referred to simply as "welfare," represent only a small fraction of social welfare spending,[1] and they raise fewer people out of poverty than do such social insurance programs as Social Security and Unemployment Insurance (Marmor, Mashaw, and Harvey 1990). Nevertheless, both public thinking and elite debates over government antipoverty policy focus disproportionately on these means-tested programs. In this article the term *welfare* is used in this narrow sense, to refer to means-tested transfer programs that provide benefits directly to individuals.

## SOURCES OF OPPOSITION TO WELFARE

Previous efforts to explain Americans' opposition to welfare often have pointed to the economic self-interest of middle-class taxpayers. From this perspective, support for welfare is more likely to be found among lower income Americans who are welfare recipients, who have friends or family receiving welfare, or who envision the possibility of needing welfare in the future. Greater opposition to welfare is more likely among middle- and high-income taxpayers, who resist paying taxes for programs from which they do not expect to benefit.

The economic self-interest explanation is widely assumed to be true, and debates over public policy often rest on the assumption that the middle class resents paying for programs that benefit only the poor (e.g., Jencks 1992; Skocpol 1990, 1991). This explanation for opposition to welfare is intuitively plausible and is bolstered by survey analyses showing that support for welfare is greatest among the least well off (AuClaire 1984, Cook and Barrett 1992, Curtin and Cowan 1975, Hasenfeld and Rafferty 1989, Kluegel and Smith 1986, Williamson 1974). But this research also shows that the association of economic status and welfare attitudes is quite modest; many poor Americans oppose welfare, and many who are well off support it, despite their apparent self-interest in cutting welfare spending.[2]

The second widespread explanation for opposition to welfare focuses on individualism. A belief in personal effort and responsibility, and a suspicion of government, have long been viewed as dominant elements of American political culture (Tocqueville 1969 [1835], Hartz 1955, Lipset 1979, McClosky and Zaller 1984, Williams 1956). Along with self-interest, the "culture of individualism" account of antiwelfare sentiment figures prominently in policy debates (e.g., Marmor, Mashaw, and Harvey 1990, Mead 1986). Once again, this popular belief has received support from surveys of public attitudes. In a nuanced study of individualism and the welfare state, Feldman and Zaller (1992) argue that individualist values constitute the most common ideological orientation upon which Americans draw in thinking about social welfare. The analysis by Feldman and Zaller is consistent with that of other researchers who have found that respondents with a greater commitment to individualist beliefs express greater opposition to welfare (Feagin 1975, Hasenfeld and Rafferty 1989, Kluegel and Smith 1986, Williamson 1974).

In contrast to the attention devoted to economic self-interest and individualism, racial attitudes have been largely neglected in prior studies of welfare views. Most research, including some of the most recent and ambitious analyses, has completely omitted racial attitudes from models of welfare support (Alston and Dean 1972, AuClaire 1984, Cook and Barrett 1992, Curtin and Cowan 1975, Feagin 1975, Hasenfeld and Rafferty 1989, Williamson 1974).[3] Evidence has begun to accumulate, however, that indicates the importance of racial attitudes in shaping opposition to welfare. A number of studies provide hints at this association (Kluegel and Smith 1986, Sears and Citrin 1985, Smith 1987), but they were not designed specifically to assess the influence of racial attitudes on welfare views and in this regard suffer from a variety of shortcomings (see Gilens 1995b for a more detailed discussion of this literature).[4]

More recently, I explicitly set out to assess the importance of whites' racial attitudes in shaping opposition to welfare (Gilens 1995b). This analysis showed that the dimension of racial attitudes with the strongest effect on welfare views is the extent to which blacks are perceived as lazy, and this perception is a better predictor of welfare attitudes than such alternatives as economic self-interest, egalitarianism, and attributions of blame for poverty. Like other analyses, however, this effort failed to resolve fully an important difficulty in dealing with this topic: the substantial overlap in the public mind between "African Americans" and "poor Americans." Blacks compose a disproportionate share of the poor in the United States (currently about 28%; U.S. Bureau of the Census 1995). More to the point, the public *thinks* that blacks make up an even larger share of the poor. In one national survey, respondents were asked: "Of all the people who are poor in this country, are more of them black or are more of them white?"[5] Fifty-one percent of respondents chose black, and only 17% white (the remainder said about equal or had no opinion). In the National Race and Politics Study analyzed below, respondents were simply asked: "What percent of all the poor people in this country would you say are black?" The median guess was 50%.[6]

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[1] In 1990, approximately $42 billion was spent on AFDC, Food Stamps, and General Assistance by all levels of government. This constituted about 7% of the $613 billion spent on social welfare. Total social welfare spending includes social insurance ($419 billion alone), health care, veterans' programs, education, housing, and public aid (U.S. Bureau of the Census 1993, tables 579, 583).

[2] For example, Hasenfeld and Rafferty (1989) find a standardized regression coefficient of −0.22 for the relationship between family income and support for welfare, while Kluegel and Smith (1986) find an analogous coefficient of −0.14.

[3] As have public opinion analyses, studies in political development also have neglected the role of race in shaping the U.S. welfare state. The most significant exceptions are Quadagno (1988, 1994) and Lieberman (1995).

[4] In addition to these analyses, Bobo and Kluegel (1993) conclude that racial attitudes are important in shaping white opinion toward "opportunity enhancing" policies, such as enterprise zones and educational spending, but relatively unimportant in shaping attitudes toward "equal outcomes" policies. Bobo and Kluegel did not use support for welfare as a measure of an equal outcome policy, however, but used instead the more ambiguous measures of "government efforts to improve the standard of living of the poor" and "government spending to assist the poor." (These could take many forms, including opportunity enhancement, such as job training). At any rate, Bobo and Kluegel focused on the differences between income-targeted versus race-targeted policies, not on the relative importance of different factors in shaping attitudes in any given policy domain.

One consequence of the overlap between black and poor in the public's thinking is that attitudes toward the poor are confounded with attitudes toward blacks. Thus, a key obstacle to confident assessment of the importance of racial views in shaping welfare attitudes is the ability to distinguish between these two sets of influences. Both must be included as independent variables in any analysis, and the measures employed must be as nearly parallel as possible.[7] In the research reported here, this is achieved by using a randomized survey-based experiment to disentangle whites' attitudes toward blacks and toward the poor.

The major hypothesized influences on welfare views consist of economic self-interest, individualism, racial attitudes, and attitudes toward the poor in general. But two additional factors may shape welfare attitudes. First, partisan identification has long been recognized as an important influence on political policy preferences, and previous research has shown that Republicans express greater opposition to welfare than do Democrats (Cook and Barrett 1992). Similarly, conservatives are more likely to oppose welfare than are liberals (Cook and Barrett 1992). Consequently, both partisanship and ideological self-identification are taken into account in examining welfare views.

## DATA AND METHODS

The data analyzed here come from the 1991 National Race and Politics Study, a nationwide random-digit telephone survey directed by Paul M. Sniderman, Philip E. Tetlock, and Thomas Piazza and conducted by the Survey Research Center at the University of California at Berkeley. The survey produced 2,223 completed interviews, with a response rate of 65.3%. In addition, a mailback questionnaire was obtained from 1,198 of those respondents who completed telephone interviews. The response rate for the mailback portion of the study was 35.2% of the total eligible sample units and 53.9% of those completing the telephone interview.[8]

Nonresponse analysis was conducted to assess the degree to which the mailback sample was representative of the full study sample. No differences were found on any of the attitude variables used in this research. The samples do differ somewhat in income and education, but these small differences are further reduced when the appropriate sample weights are applied. (See Appendix B for comparisons of sample and population demographics.)

Black respondents are excluded from all the analyses presented below because racial attitudes clearly play different roles in shaping policy preferences among blacks and nonblacks. Separate analyses would be necessary to understand the welfare attitudes of blacks, which would be impossible with only 201 black respondents to the telephone survey. (For analyses of blacks' welfare attitudes, see Sigelman and Welch 1991 and Gilliam and Whitby 1989.) This analysis, then, will draw upon the 2,022 nonblack respondents to the telephone survey and the subsample of 1,137 nonblacks who also completed the mailback questionnaire.

As in previous research, economic self-interest will be measured by family income. Although commonly used as an indicator of self-interest, income is clearly not an ideal measure. First, income represents only a "snapshot" of a respondent's economic status; recent changes in income or future expectations may be more directly tied to perceptions about potential benefits from welfare or burdens from taxes. Second, income represents an objective measure of a respondent's social condition but does not directly tap his or her *perceptions* of the potential personal economic costs and benefits of welfare or the likelihood that those costs or benefits will be realized. Despite these limitations, family income has been shown to be the best objective measure of economic condition for the purpose of assessing welfare views (Gilens 1995b) and is virtually the only such measure used in previous studies of this topic (Cook and Barrett 1992 provide the lone exception).[9]

Individualism concerns the proper balance of personal and government responsibility and was measured quite straightforwardly in the survey by the following question: "The government in Washington tries to do too many things that should be left up to individuals and private businesses." Responses were expressed on a four-point agree/disagree scale. (The full question wording, means, and standard deviations for all variables are found in Appendix A.)

Perceptions of blacks as lazy are assessed by the difference in respondents' scores on two items from a series of personal characteristics applied to blacks. The series is introduced as follows:

> Now I'll read a few words that people sometimes use to describe blacks. Of course, no word fits absolutely everybody, but, as I read each one, please tell me using a number

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[5] CBS/New York Times Survey, March 1982.

[6] It appears that Americans also exaggerate the proportion of blacks among the general population. A Gallup survey asked: "What percent of the American population today would you say is black?" The median response was about 32% (Gallup Poll, March 15–18, 1990). To some extent, then, the perception that blacks constitute half or more of America's poor may reflect this more general misperception about the number of blacks in the United States.

[7] If the measures employed tap different dimensions of attitudes toward blacks and toward the poor, or if they elicit different response characteristics due to differing question wording or formats, then they may not fully satisfy their role as control variables for each other.

[8] Because many of the survey items of interest here (including the questions composing the welfare attitudes index) are from the mailback questionnaire, the analyses are often limited to the 1,198 respondents who completed it. Separate weights were developed by the Survey Research Center for analyses based on the mailback sample, to permit greater confidence in generalizing from this sample.

[9] In addition to these considerations, the use of a single continuous variable to measure income in a regression analysis reflects only its *linear* relationship with welfare attitudes. Other analyses, however, have shown that allowing for a nonlinear relationship does not increase the power of income as a predictor of welfare views (Gilens 1995b).

> from 0 to 10 how well you think it describes blacks as a group. If you think it's a very good description of most blacks, give it a 10. If you feel a word is a very inaccurate description of most blacks, give it a 0.

The measure of blacks as lazy was constructed by subtracting a respondent's score for "hardworking" from his or her score for "lazy."[10]

Two questions tapped perceptions of the work ethic of the poor. The first asked: "Most people who don't succeed in life are just plain lazy." Responses were given on a four-point agree/disagree scale. The second asked whether most poor people are poor because "they don't try hard enough to get ahead" or because "they don't get the training and education they need."[11]

Partisanship was measured with the standard seven-point scale ranging from strong Democrat to strong Republican, while liberal/conservative self-placement consisted of three categories—liberal, conservative, and either moderate or "haven't thought much about it." Educational attainment was measured on a six-point scale, ranging from "eighth grade or lower" to "some graduate work or graduate degree," and age was measured in years.

Respondents' welfare views were measured by (1) a single variable asking whether federal spending for welfare should be increased, decreased, or kept the same and (2) an index of four agree/disagree questions measuring respondents' attitudes toward welfare (see Appendix A for full text).[12] The first of these measures had a more economic focus and indicated respondents' policy preferences with regard to welfare expenditures, while the latter reflected how positively or negatively they viewed welfare in the abstract and welfare recipients in particular. Employing these two alternative measures allowed confirmation of the basic findings concerning race and helped interpret some of the other dynamics of public attitudes toward welfare.[13]

Like most previous analyses of welfare attitudes, the statistical models used here indicate the effect of each predictor variable net of all the other predictors in the model. Such models reflect only the direct effect of each independent variable, and not any influence that may be channeled through other independent variables in the model. As is often the case, however, many of the predictor variables used here may plausibly be considered endogenous: Beliefs about blacks, beliefs about the poor, ideology, partisan identification, and individualism may be influences on, and influenced by, one another. Any causal relationships among the predictors would imply that the independent variables may have both a direct effect on welfare views (which the model captures) and an indirect effect channeled through the other predictors (which the model does not capture).

Because the causal relationships among the predictor variables are ambiguous, it is impossible to assess fully the magnitude of any indirect effects. For example, the association of liberal/conservative ideology and racial views may reflect the influence of each upon the other. Without knowing the causal direction of this relationship it is impossible to determine whether (and to what extent) ideology has an indirect effect on welfare attitudes channeled through racial views, or racial views have an indirect effect on welfare attitudes channeled through ideology. By ignoring such indirect effects, the models below avoid improperly crediting an independent variable with a larger total effect than is warranted. At the same time, the direct effects reported below must be considered estimates of the minimum influence of each predictor on welfare attitudes, with the possibility that the total effect (i.e., direct plus indirect effects) could be larger.

Although causally ambiguous relationships among predictor variables are often found in cross-sectional survey analyses, unmeasured indirect effects only become problematic to the degree that the predictor variables are strongly associated with one another. If these associations are weak, then any indirect effects also must be weak. In the analyses reported below, the correlations among the predictors are low enough that indirect effects are not likely to alter the results significantly.[14]

## FINDINGS

I first estimate a conventional structural model of welfare views. This model resembles those found in earlier research (e.g., Cook and Barrett 1992, Hasenfeld and Rafferty 1989, Kluegel and Smith 1986) but includes more adequate measures of racial attitudes. Table 1 shows OLS estimates for the following model:

$$W = a + b_1\mathrm{BL} + b_2\mathrm{PL} + b_3\mathrm{I} + b_4\mathrm{F} + b_5\mathrm{A} + b_6\mathrm{P} + b_7\mathrm{L} + b_8\mathrm{E} + e,$$

where W = welfare views, BL = belief that blacks are lazy, PL = belief that poor people are lazy, I = individualism, F = family income, A = age, P = partisan identification, L = liberal/conservative self-placement, and E = education. For these analyses, welfare views are scored from −1 for the most negative attitudes to +1 for

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[10] Cronbach's alpha for the scale measuring perceptions of blacks as lazy was 0.35. Blacks were the only group asked about. The full list of characteristics (in the order asked) was: dependable, intelligent in school, aggressive or violent, lazy, smart with everyday things, law abiding, boastful, determined to succeed, hardworking, friendly, irresponsible, keep up property, complaining, and good neighbors. Of these characteristics, "lazy" and "hardworking" were chosen to measure perceptions of blacks' commitment to the work ethic both because of their face validity and because these terms are the negative and positive characteristics most strongly associated with respondents' preferences on welfare spending and with their welfare attitudes.

[11] Cronbach's alpha for the scale measuring perceptions of the work ethic of the poor was 0.38.

[12] Cronbach's alpha for the welfare attitudes scale was 0.61.

[13] Geographical region also was examined (using the National Election Study's standard four-region classification). Based on an F-test for the full set of region variables, region was found to be nonsignificant as a predictor of the three dependent variables used below. Consequently, region was dropped from the analysis.

[14] The matrix of 28 correlations among the eight predictors shown in Table 1 reveals only three correlations above 0.15 (education and income are correlated at 0.37, partisanship and ideology at 0.22, and education and racial attitudes at 0.21).

**TABLE 1. Predictors of Whites' Welfare Spending Preferences and Welfare Attitudes**

| | Welfare Spending | | | Welfare Attitudes | | |
|---|---|---|---|---|---|---|
| | *b* | (standard error) | *p* | *b* | (standard error) | *p* |
| $b_1$ Blacks are lazy | −.67 | (.13) | <.001 | −.55 | (.06) | <.001 |
| $b_2$ Poor people are lazy | −.47 | (.10) | <.001 | −.31 | (.04) | <.001 |
| $b_3$ Individualism | −.29 | (.08) | <.001 | −.22 | (.04) | <.001 |
| $b_4$ Family income | −.28 | (.08) | .001 | −.01 | (.04) | ns |
| $b_5$ Age | −.30 | (.11) | .004 | .06 | (.05) | ns |
| $b_6$ Party identification (high = Democratic) | .17 | (.05) | <.001 | .04 | (.02) | ns |
| $b_7$ Liberal/conservative (high = liberal) | .10 | (.06) | ns | .09 | (.03) | .001 |
| $b_8$ Education | .04 | (.11) | ns | .11 | (.05) | .030 |
| $R^2$ | | .11 | | | .22 | |
| *N* | | 1017 | | | 1011 | |

ns = not significant

*Note*: All independent variables are recoded to a range of 0 to 1. "Welfare spending" and "welfare attitudes" are scored from −1 for most negative views to +1 for most positive views. (See Appendix A for question wording and index construction.)

*Source*: All data are from the National Race and Politics Study telephone survey except for the "welfare attitudes" and "poor people are lazy" indices, which are from the mailback questionnaire to the National Race and Politics Study.

the most positive, and all predictor variables are rescored to a 0–to–1 scale.[15]

With regard to both welfare spending preferences and the welfare attitudes index, the results in Table 1 show that racial attitudes strongly influence whites' welfare views. Whites who fully accepted the view of blacks as lazy had predicted scores on welfare spending that were 0.67 units lower than whites who fully rejected this view. This same difference in racial attitudes was associated with a unit change of 0.55 in score on the welfare attitudes index.

After racial attitudes, the predictor with the strongest relationship to welfare views was the belief that poor people are lazy. A one-unit change in respondents' perception of the work ethic of the poor was associated with a unit change of 0.47 in welfare spending preferences and of 0.31 in the welfare attitudes index. Individualism was more weakly associated with welfare views. A one-unit change in individualism (from strong disagreement to strong agreement) was associated with a unit change of 0.29 in welfare spending preference and of 0.22 in the welfare attitudes index.

Economic self-interest, as measured by higher family income, was associated with greater opposition to welfare spending but was not related to the welfare attitudes index. This difference supports the validity of income as an indicator of economic self-interest, as it appears that whites with higher incomes (and therefore higher taxes) do not hold more negative views of welfare or welfare recipients, but nevertheless they are more likely to favor cuts in welfare spending. Even with regard to welfare spending, however, the effect of family income was only moderate: A difference of $60,000 in income (between the lowest and the highest categories) was associated with a unit difference of 0.28 in welfare spending.

Among the hypothesized sources of opposition to welfare, the perception that blacks are lazy was clearly the most important, followed by perceptions of poor people and individualist opposition to government activity in general. Economic self-interest was an important influence on welfare spending preferences but was unrelated to welfare attitudes more generally. In addition to these four influences on welfare views, it was found that older respondents were more opposed to welfare spending but did not differ in their scores on the welfare attitudes index. This suggests that age influences welfare policy preferences not because older whites hold different attitudes toward the poor but because they see their self-interest as better served by cutting welfare spending.

The remaining independent variables in Table 1 are more remarkable for their *lack* of association with welfare views. For example, although the relationship of party identification with welfare spending preferences was statistically significant, the association was quite weak; strong Democrats were only 0.17 units more supportive of welfare spending than were strong Republicans. Similarly, liberals were only 0.10 units higher on the welfare spending measure than were conservatives. Opposition to welfare, it seems, cuts across partisan and ideological lines, turning instead on beliefs about the character of blacks and of poor people, commitment to individualism, and perceptions of economic self-interest.[16]

Thus far, we have seen that whites' welfare views were strongly influenced by their perceptions of blacks, and thus the popular belief that welfare is a "race coded" issue appears warranted. Whatever other reasons whites may have for opposing welfare, their negative views of blacks appear to constitute an important factor in generating that opposition.

It is, perhaps, no great shock to find that the belief that blacks are lazy can generate opposition to welfare among whites. But two things must be kept in mind to appreciate fully the importance of this finding. First, it was not poor blacks, or blacks on welfare, who were asked about; respondents were instructed to indicate how well "lazy" and "hardworking" describe *most blacks*. This suggests not only that whites' evaluations of welfare are steeped in racial considerations but also that the relevant racial views are not limited to perceptions of poor blacks or blacks on welfare but extend to the much broader category of American blacks in general.[17]

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[15] Predictor variables were rescored via a linear transformation. For each variable, a constant was subtracted from all scores so that the lowest category equaled zero; the scores then were divided by the range for that variable.

[16] The effects of partisanship and ideology were weak not only in the multivariate models in Table 1 but also at the zero-order. Thus, the lack of a strong relationship between these variables and welfare views is not due to their collinearity or to their indirect effects on welfare views.

The second thing to keep in mind is that the influence of racial views on welfare support is even stronger than the effect of views about the poor in general. Remarkably, whites' perceptions of blacks as lazy appear more important in shaping opposition to welfare than do their perceptions of poor people as lazy. Once again, this suggests that the welfare debate has become so racialized that what matters most to the white public is perceptions of a single subgroup of welfare recipients—blacks. Although 63% of current welfare recipients are nonblack (U.S. House of Representatives 1994), beliefs about blacks appear to dominate whites' thinking when it comes to evaluating welfare.

## MEASURING RACIAL ATTITUDES WITH A SURVEY-BASED EXPERIMENT

While the findings in Table 1 are dramatic, one shortcoming in the data limits our confidence in the results: Perceptions of the work ethic of blacks and of the poor were not assessed with identical questions. In the first case, respondents were asked to rate the applicability of "lazy" and "hardworking" to "most blacks"; in the second case, questions asked whether "most people who don't succeed in life are just plain lazy" and whether most poor people are poor because "they don't try hard enough to get ahead" or because "they don't get the training and education they need."

Although these questions appear to be valid measures of perceptions of blacks and of the poor, we would prefer to have strictly identical measures to assess their relative importance in shaping welfare attitudes. Unfortunately, respondents cannot simply be asked to evaluate both blacks and poor people using the same questions due to consistency pressures that exist within the survey setting (Schuman and Presser 1981). Because social norms of equality apply to racial issues, respondents may feel pressured to provide the same response to questions about blacks and about whites. Thus, if white respondents acknowledge their belief that black welfare recipients are lazy, on a subsequent question they face a pressure to say that white welfare recipients are lazy as well. Alternatively, if they first indicate that whites on welfare would prefer to be working, they may feel pressured to respond that black welfare recipients would rather be working, too. An apparent example of such consistency pressures with regard to racial attitudes comes from a recent national telephone survey in which respondents were asked to indicate how "warm" or "cold" they felt toward nine different social groups on a "feeling thermometer" of 0 to 100 points. Fully 67% gave *exactly the same score* to blacks and whites on this 100-point scale.[18]

In order to overcome the consistency pressures inherent in asking the same respondents to evaluate poor blacks and poor whites using identical questions, I employed an experimental manipulation whereby half the respondents were asked only about blacks and the other half an identical question with reference to whites. Because each respondent was asked only about one racial group, consistency pressures were absent. If white respondents' thinking about poverty and welfare is dominated by their images and evaluations of blacks, then we would expect the black version of this experimental question to be more strongly related to overall welfare views than the white version. By means of this experiment we can reveal something of the thought process or judgmental heuristic (Sniderman, Brody, and Tetlock 1991) respondents use in evaluating welfare. When whites assess welfare in general, or identify their preferences with regard to welfare spending, are they thinking more about black welfare recipients, white welfare recipients, or both equally?

Use of this technique combined the advantages of the randomized experiment with those of the sample survey (see Piazza, Sniderman, and Tetlock 1989). Randomly assigning respondents to question "treatments" ensured that differences in responses resulted from differences in the questions asked, and since the random assignment was uncorrelated with respondents' characteristics, we need not worry that differences in responses to poor blacks and poor whites would be confounded with other factors, such as income, education, or political attitudes. By embedding this experiment in a large-scale national survey, we also retained the ability to generalize to the U.S. population, an ability that is severely limited in the typical small-scale experiment.

In the "welfare mother" experiment, respondents were asked their impressions of a welfare recipient described as either a black or white woman in her early 30s, who has a ten-year-old child, and who has been on welfare for the past year.[19] Respondents were asked, first, how likely it is that the woman described will try hard to find a job and, second, how likely it is that she will have more children to get a larger welfare check (see Appendix A for question wording). For this analysis, the responses to the two questions are combined into an index of beliefs about welfare mothers,[20] with those who said it is very unlikely that the welfare mother will look

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[17] Poor blacks constitute about 33% of all American blacks; blacks receiving AFDC make up 23% of all black families (U.S. Bureau of the Census 1993, 1995; U.S. House of Representatives 1994).

[18] These data are from the 1994 Multi-Investigator Study directed by Paul M. Sniderman, Henry E. Brady, and Philip E. Tetlock and conducted by the Survey Research Center at the University of California at Berkeley, with support from the National Science Foundation (SBR 9309946). This random-digit telephone survey of the United States had 1,464 cases and a response rate of 65.5%.

[19] For half the respondents, the welfare mother was described as a high school dropout, for the other half as a high school graduate. In the current analysis, these two versions of the question are combined.

[20] Cronbach's alpha for the welfare mother index was .61.

**TABLE 2. Beliefs about Black and White Welfare Mothers as Predictors of Whites' Welfare Views**

| | Welfare Spending | | | Welfare Attitudes | | | Poor People Are Lazy | | |
|---|---|---|---|---|---|---|---|---|---|
| | *b* | (standard error) | *p* | *b* | (standard error) | *p* | *b* | (standard error) | *p* |
| $b_1$ Perceptions of welfare mothers | .46 | (.07) | <.001 | .38 | (.04) | <.001 | .23 | (.06) | <.001 |
| $b_2$ Race of welfare mother | .01 | (.04) | ns | .04 | (.02) | ns | .01 | (.03) | ns |
| $b_3$ Interaction: perceptions × race | .28 | (.13) | .040 | .21 | (.08) | .010 | .21 | (.11) | .050 |
| $b_4$ Family income | −.26 | (.06) | <.001 | .04 | (.04) | ns | −.15 | (.05) | .005 |
| $b_5$ Individualism | −.22 | (.06) | <.001 | −.24 | (.04) | <.001 | −.21 | (.05) | <.001 |
| $b_6$ Age | −.25 | (.08) | .001 | −.07 | (.05) | ns | .08 | (.07) | ns |
| $b_7$ Party identification (high = Democratic) | .17 | (.04) | <.001 | .07 | (.02) | .001 | .07 | (.03) | .02 |
| $b_8$ Liberal/conservative (high = liberal) | .14 | (.04) | .001 | .12 | (.03) | <.001 | .11 | (.04) | .004 |
| $b_9$ Education | .10 | (.08) | ns | .15 | (.05) | .001 | .16 | (.07) | .020 |
| $R^2$ | | .07 | | | .18 | | | .07 | |
| *N* | | 1913 | | | 1107 | | | 1100 | |

ns = not significant

*Note*: All independent variables are recoded to a range of 0 to 1, except perceptions of welfare mothers and race of welfare mother, which are coded −0.5 to +0.5. "Welfare spending," "welfare attitudes," and "poor people are lazy" are all scored from −1 for the most negative attitudes toward poverty and welfare to +1 for the most positive attitudes. See Appendix A for question wording and index construction. The larger sample size for the "welfare spending" model is due to the inclusion of only variables from the telephone survey.

*Source*: All data are from the National Race and Politics Study telephone survey except for the "welfare attitudes" and "poor people are lazy" indices, which are from the mailback questionnaire to the Race and Politics survey.

for a job and very likely that she will have more children receiving the lowest scores.

To assess the relative importance of perceptions of black and white welfare mothers, I reestimated the models shown in Table 1 using responses to the welfare mother experiment in place of the measures of attitudes toward blacks and toward the poor.[21] (I also applied this same model to the belief that poor people are lazy, which was used as an independent variable in Table 1.) Our interest in these analyses is the interaction between respondents' perceptions of welfare mothers and the manipulated race of the welfare mother in question. That is, does identifying the welfare mother as black increase the relationship between the associated perception and whites' welfare attitudes and welfare policy preferences? To answer this question I estimated the following model:

$$W = a + b_1M + b_2R + b_3(M^*R) + b_4F + b_5I + b_6A + b_7P + b_8L + b_9E + e,$$

where W = welfare views, M = perceptions of welfare mothers, R = race of the welfare mother in question, F = family income, I = individualism, A = age, P = partisan identification, L = liberal/conservative self-placement, and E = education.

Interaction terms in multiple regression (such as M*R above) are often highly collinear with their component variables, resulting in large standard errors and low significance levels. The standard remedy for this condition is to "center" the component variables about their means before constructing the interaction term (Cronbach 1987; Jaccard, Turrisi, and Wan 1990). This centering reduces the standard errors but does not affect the magnitude of the interaction coefficient or of the calculated slopes for the component variables (in this case, the influence of perceptions of black and white welfare mothers).

Following this procedure, perceptions of welfare mothers were scored from −0.5 for the most negative views to +0.5 for the most positive, and the race of the welfare mother in question was scored −0.5 for whites and +0.5 for blacks. Of particular interest is the coefficient for the interaction term, $b_3$. If respondents are thinking more about black welfare mothers when they offer their overall views of welfare, then we would expect the black version of the welfare mother experiment to be more strongly related to welfare views than the white version, and $b_3$ will be positive. Table 2 shows that this is indeed the case; in all three models of welfare views, $b_3$ is positive in sign and statistically significant.

In order to assess the difference in effect of perceptions of black and white welfare mothers, I next calculated the estimated effect of welfare mother perceptions for respondents assigned to the "black" and "white" treatment conditions. For the white treatment condition, this effect was calculated as:

$$b_{\text{white}} = b_1 + b_3(-0.5);$$

for the black condition the analogous calculation was:

$$b_{\text{black}} = b_1 + b_3(0.5).$$

These results (shown in Table 3) indicate that the influence of beliefs about welfare mothers is about twice as strong when she is black than when she is white. For example, a one-unit change in beliefs about white wel-

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[21] The analyses in Table 2 do not include the "blacks are lazy" and "poor people are lazy" indices from Table 1 because they are alternative measures of the same underlying views about the commitment to the work ethic of poor blacks and nonblacks that the welfare mother questions are designed to tap.

**TABLE 3. Effect of Beliefs about Black and White Welfare Mothers on Whites' Welfare Views**

| | Welfare Spending | Welfare Attitudes | Poor People Are Lazy |
|---|---|---|---|
| Perceptions about white welfare mothers | .32 | .28 | .13 |
| Perceptions about black welfare mothers | .60 | .49 | .34 |

*Note*: Table entries indicate the effect of a one-unit change in perceptions of welfare mothers (scored −0.5 to +0.5) on whites' welfare views (scored −1 to +1), holding constant the other variables in Table 2. Entries are calculated from results in Table 2 as follows: For white welfare mothers entries above = $b_1$ + (−0.5)($b_3$); for black welfare mothers entries above = $b_1$ + (0.5)($b_3$).

fare mothers (i.e., from the most negative to the most positive views) is associated with a unit increase of 0.32 in support for welfare spending; the same one-unit change in perceptions of black welfare mothers is associated with a unit increase of 0.60. Similarly, in predicting both the welfare attitudes index and the belief that poor people are lazy, we find that white respondents' views of white welfare mothers have a much smaller effect than do their views of black welfare mothers. These differences in the influence of the black and white versions of the welfare mother question are both large and consistent across the three different measures of attitudes toward welfare and toward the poor. Using strictly parallel questions, it is clear that whites consider black welfare recipients much more strongly than white welfare recipients when evaluating welfare.

The dramatic differences in the effect of perceptions of black and white welfare mothers indicate that the widespread intuition about the "race-coded" nature of contemporary welfare politics is correct; white Americans' welfare views are clearly not "race-neutral" expressions of their economic self-interest, commitment to individualism, or evaluations of poor people in general. Instead, those views are strongly rooted in their beliefs about blacks, and particularly their perceptions of black welfare recipients.

Before fully accepting the findings in tables 2 and 3, however, we must examine the possibility that the greater predictive power of the black version of the welfare mother experiment could result from differences in the distribution of attitudes in response to the two different versions. In fact, however, the distribution of responses to the two versions was remarkably similar. With the welfare mother perception index scored −0.5 for the most negative views and +0.5 for the most positive views, the black and white versions of this index had means of −0.07 and −0.05, respectively, with standard deviations of 0.258 and 0.261. When black and white welfare mothers were described in similar terms, white respondents made nearly identical evaluations.[22]

The "racialization" of welfare as a political issue has implications for both the character of electoral campaigns and the complexion of U.S. race relations. But does whites' tendency to focus primarily on black welfare recipients also generate greater opposition to welfare? It may appear from whites' similar perceptions of white and black welfare mothers that this is not the case. After all, if they view white welfare mothers just as negatively as black welfare mothers, it may not matter that whites' welfare attitudes are dominated by their views of blacks, at least as far as their welfare policy preferences are concerned.

But the similarity of whites' evaluations of black and white welfare mothers does not mean that racial considerations do not affect support for welfare; the *consequences* of holding negative beliefs about black and white welfare mothers are not the same. Figure 1 shows that, as expected, respondents with negative perceptions of welfare mothers expressed more opposition to welfare than did those with positive views, but there is a racial asymmetry apparent in this figure. The welfare attitudes of white respondents with positive views of black welfare mothers were the same as those with positive views of white welfare mothers (0.09 versus 0.10; $p = ns$), and the welfare attitudes of those with neutral perceptions of black and white welfare mothers were the same (0.01 versus −0.03; $p = ns$). Yet, negative beliefs about black welfare mothers were associated with significantly more negative views of welfare in general than were negative beliefs about white welfare mothers (−0.25 versus −0.10; $p < 0.001$).

Whether whites base their evaluations of welfare on their images of black or white welfare recipients does matter, *both* because a racially charged welfare debate has important implications for the character of U.S. politics *and* because whites' negative evaluations of black welfare recipients appear to generate greater opposition to welfare. As Figure 1 reveals, holding negative perceptions of white welfare mothers leads to increased opposition to welfare among whites, but the increase is modest; holding negative views of black welfare mothers, in contrast, leads to a substantial increase in opposition.

We saw above that racial attitudes figured prominently in whites' evaluations of welfare and that perceptions of black welfare mothers were stronger influences on welfare views than were perceptions of white welfare mothers. Figure 1 reveals just why this is so. The greater power of beliefs about black welfare mothers in shaping white respondents' welfare views comes from the particular salience of *negative* images of blacks.

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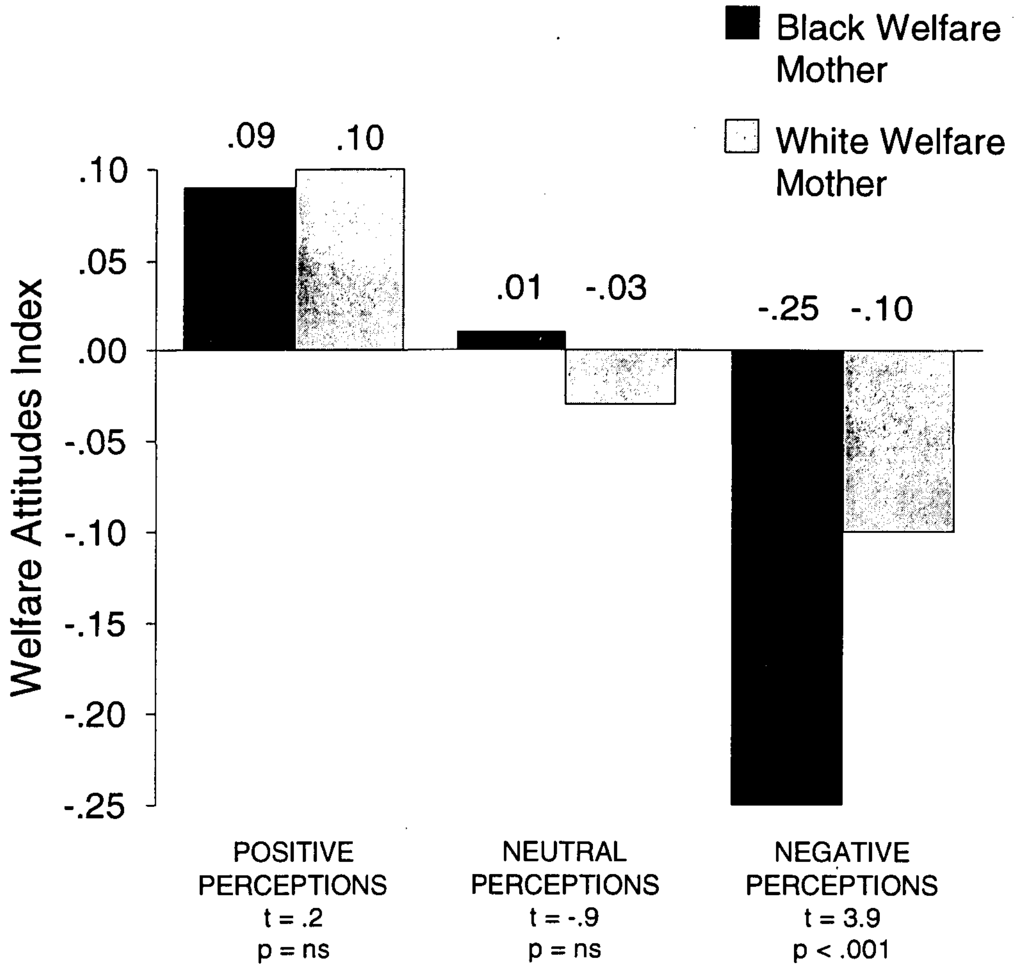
[22] On balance, white respondents expressed only slightly negative views of black and white welfare mothers. The lack of a more negative response may reflect the description of the welfare mother in this experiment: She is 30 years old, has one ten-year-old child, and has been on welfare for only one year. In addition, for a random half of the respondents, the welfare mother is described as being a high school graduate (see footnote 19). This hypothetical welfare mother is in fact older, has fewer children, and has been on welfare for a shorter time than the average current AFDC recipient (U.S. House of Representatives 1994).

**FIGURE 1. Welfare Attitudes of Whites, by Perceptions of Black and White Welfare Mothers**



*Source*: Welfare mother stereotypes are from the National Race and Politics Study telephone survey; the welfare attitudes index is from the mailback questionnaire to the National Race and Politics Study.

## CONCLUSION

This article examined a simple but important question: Do white Americans' racial attitudes significantly shape the positions they hold on ostensibly race-neutral social policies such as crime, welfare, immigration, and illegal drugs? Do these issues—as many suspect—engage whites' dislike, resentment, or fear of blacks? We looked only at whites' attitudes toward welfare, and we cannot extend these findings to the other issues in this domain. But with regard to welfare, the answer is clear: Racial attitudes are a powerful influence on white Americans' welfare views. Indeed, the analyses reported here suggest that racial considerations are the single most important factor shaping whites' views of welfare.

The dramatic effect of racial attitudes on welfare views should not be taken to mean that other factors are absent. On the contrary, this analysis indicates that beliefs about the nonblack poor as well as individualism, partisan identification, liberal/conservative ideology, age, income, and education all influence whites' thinking about welfare. Nor should the association between racial attitudes and welfare views be understood apart from its historical context. Just as Americans now associate poverty with blacks, other social groups appear to have dominated public thinking about the poor in other historical periods. During the 19th century, Irish immigrants were viewed as shiftless, drunken, and happy to live off handouts (Harrington 1984), while in the early 1960s a more sympathetic poverty discourse arose focusing on the white poor of Appalachia (Katz 1989).

To some degree these shifts follow the demographic changes in U.S. poverty. Currently, blacks are substantially overrepresented among America's poor, and this is clearly an important element in the public's association of race and poverty. But blacks in this country have always been disproportionately poor. In the early 1960s, when poverty was more associated with rural whites than urban blacks, blacks were even more overrepresented among the poor (and among welfare recipients) than they are today.[23] Furthermore, while blacks account for a disproportionate number of welfare recipients, they exert an even more disproportionate influence on white Americans' welfare views. Although there are more whites among welfare recipients than there are blacks,[24] beliefs about blacks in general, and black welfare mothers in particular, are substantially more important in shaping whites' welfare views than are beliefs about the poor or perceptions of white welfare mothers. Thus, the demographics of poverty cannot fully account for whites' tendency to think about welfare in racial terms. Instead, this tendency must be understood as the product of a particular social and cultural context within which blacks' overrepresentation among the poor acquires an exaggerated salience for white Americans.

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[23] In 1960 blacks accounted for 26% of poor people, 2.5 times their proportion of the U.S. population (U.S. Bureau of the Census 1993, 469; 1995, 14). Blacks now account for 28% of the poor, or 2.2 times their proportion of the population (U.S. Bureau of the Census 1995, 480, 14). Similarly, in 1960 blacks constituted 40% of AFDC recipients, 3.8 times their proportion of the population (Piven and Cloward 1993,

Welfare spending represents a small fraction of all government spending, but its symbolic power far exceeds its fiscal importance. Debates over welfare, and promises of reform, have figured prominently in recent presidential campaigns and appear destined to play an important role in national politics. The analyses above suggest that the symbolic power of welfare as a political issue stems in large measure from its racial undertones.

This finding has troubling implications for electoral politics. When crime or welfare become vehicles with which to mobilize and stimulate antiblack sentiments among the white electorate, we face an insidious politics of racial division. School integration and affirmative action are also difficult and divisive issues, but in those cases the racial component is explicit. While discussion of these issues can be painful, racial claims can be challenged, and debate can be subjected to the norms of "civilized discourse." But "race-coded" issues are attractive to some politicians precisely because they can exploit the power of racial suspicion and animosity while insulating themselves from charges of race-baiting.[25]

When explicit claims about race are made, they can be rebutted; but when blacks are linked with crime, welfare, or drug use only implicitly, such links are less likely to be challenged. Thus, a subterranean discourse on race in U.S. society emerges, based largely on misleading images and chosen to influence voters by inciting fear or indignation. Rarely does one hear public figures make the explicit claim that irresponsible black mothers are the "problem" with welfare or that violence-prone black men are the reason our streets are unsafe at night. But since they are not being made, such claims are not refuted. The public is left to draw its own conclusions, based on existing stereotypes and biased media coverage,[26] and the conclusions drawn are exactly what one would expect under such conditions.

Although blacks represent only 37% of welfare recipients, perceptions of black welfare mothers dominate whites' evaluations of welfare and their preferences with regard to welfare spending. Thus, the "unspoken agenda" of racial imagery appears to be more important in shaping public understanding of welfare than are explicit debates over welfare reform that are cast in race-neutral language.

## APPENDIX A: QUESTIONS AND INDEX CONSTRUCTION

**Welfare spending** (range = −1 to +1; mean = −0.30; standard deviation = 0.77)
Suppose you had a say in making up the federal budget, would you prefer to see more spent, less spent, or the same amount of money spent on welfare as it has been?
1 = More spent, 0 = Kept the same, −1 = Less spent

**Welfare attitudes** (range = −1 to +1; mean = −0.05; standard deviation = 0.37)
Scale = (WA1 + WA2 − WA3 − WA4)/6

WA1. Most people on welfare could get by without it if they really tried.
1 = Agree strongly, 2 = Agree somewhat, 3 = Disagree somewhat, 4 = Disagree strongly
(mean = 2.33; standard deviation = 0.84)

WA2. The high cost of welfare puts too big a burden on the average taxpayer.
1 = Agree strongly, 2 = Agree somewhat, 3 = Disagree somewhat, 4 = Disagree strongly
(mean = 1.74; standard deviation = 0.76)

WA3. When people can't support themselves, the government should help by giving them enough money to meet their basic needs.
1 = Agree strongly, 2 = Agree somewhat, 3 = Disagree somewhat, 4 = Disagree strongly
(mean = 1.96; standard deviation = 0.80)

WA4. Most people on welfare would rather be working than taking money from the government.
1 = Agree strongly, 2 = Agree somewhat, 3 = Disagree somewhat, 4 = Disagree strongly
(mean = 2.45; standard deviation = 0.88)

**Blacks are lazy** (range = 0 to 1; mean = 0.45; standard deviation = 0.18)
Scale = (BL1 − BL2 + 10)/20
Now I'll read a few words that people sometimes use to describe blacks. Of course, no word fits absolutely everybody, but, as I read each one, please tell me using a number from 0 to 10 how well you think it describes blacks as a group. If you think it's a very good description of most blacks, give it a 10. If you feel a word is a very inaccurate description of most blacks, give it a 0.
BL1. Lazy (mean = 5.01; standard deviation = 2.57)
BL2. Hardworking (mean = 6.02; standard deviation = 2.11)

**Poor people are lazy** (range = 0 to 1; mean = 0.32; standard deviation = 0.25)
Scale = (PL1 − PL2 + 3)/6

PL1. What do you think makes most poor people poor? Most of them are poor because . . .
1 = They don't get the training and education they need; 2.5 = Both statements are wrong; 4 = They don't try hard enough to get ahead
(mean = 1.68; standard deviation = 1.08)

PL2. Most people who don't succeed in life are just plain lazy.
1 = Agree strongly, 2 = Agree somewhat, 3 = Disagree somewhat, 4 = Disagree strongly
(mean = 2.78; standard deviation = 0.79)

**Welfare mother perceptions** (range = 0 to 1; mean = 0.44; standard deviation = 0.26)
Scale = (WM1 − WM2 + 6)/12

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194; U.S. Bureau of the Census 1995, 14). Today blacks make up 37% of AFDC recipients, 3 times their population proportion (U.S. House of Representatives 1994, 402; U.S. Bureau of the Census 1995, 14).

[24] Thirty-seven percent of AFDC families are headed by blacks, compared with 39% headed by non-Hispanic whites and 55% by Hispanic and non-Hispanic whites combined (U.S. House of Representatives 1994).

[25] If done too crudely, such strategies risk being "exposed," just as the Willie Horton advertisement evoked outrage and charges of race-baiting from Democrats (and others). Whether this outrage diminished the ad's effectiveness is uncertain (see Mendelberg 1994).

[26] For evidence of racial bias in media coverage, see Gilens (1995a) on poverty and welfare and Entman (1992) on crime.

Now think about a (black/white) woman in her early thirties. She is a high school (graduate/drop out) with a ten-year-old child, and she has been on welfare for the past year.

WM1. How likely is it that she will have more children in order to get a bigger welfare check?
1 = Very likely, 3 = Somewhat likely, 5 = Somewhat unlikely, 7 = Not at all likely
(mean = 3.31; standard deviation = 1.83)

WM2. How likely do you think it is that she will really try hard to find a job in the next year?
1 = Very likely, 3 = Somewhat likely, 5 = Somewhat unlikely, 7 = Not at all likely
(mean = 4; standard deviation = 1.84)

**Individualism** (mean = 5.12; standard deviation = 1.69)
The government in Washington tries to do too many things that should be left up to individuals and private businesses.
1 = Disagree strongly, 3 = Disagree somewhat, 5 = Agree somewhat, 7 = Agree strongly

**Family income** (mean = 7.30; standard deviation = 3.63)
Think of the income BEFORE taxes of all members of your household living with you now.
Include income from all sources, including wages, dividends, interest, pensions, and other sources.
Responses coded in 13 categories from less than $10,000 (1) to more than $70,000 (13)

**Age** (mean = 43.4; standard deviation = 17.2)

**Partisan identification** (mean = 3.96; standard deviation = 1.94)
Generally speaking, do you usually think of yourself as a Democrat, a Republican, an Independent, or what?
Responses coded in 7 categories from Strong Republican (1) to Strong Democrat (7)

**Liberal/conservative self-placement** (mean = 1.80; standard deviation = 0.81)
Generally speaking, would you consider yourself to be a liberal, a conservative, a moderate, or haven't you thought much about this?
1 = Conservative or more like a conservative, 2 = Moderate or haven't thought much, 3 = Liberal or more like a liberal

**Education** (mean = 3.48; standard deviation = 1.21)
What is the highest grade or year of school you completed?
1 = Eighth grade or lower, 2 = Some high school, 3 = High school graduate (or GED), 4 = Some college, 5 = College graduate, 6 = Some graduate work or graduate degree

## APPENDIX B: SAMPLE AND POPULATION CHARACTERISTICS

In most respects both the full telephone sample and the mailback subsample closely resemble the U.S. population (see Table B). The raw survey data show women and young people to be slightly underrepresented in both samples, while those with no college education are substantially underrepresented. In both samples, the median income is slightly higher than that of the U.S. population. The Survey Research Center constructed sample weights reflecting the number of eligible adults in each household, the number of telephone numbers in each household, and respondents' sex, race, age, and education. As shown below, the weighted data (used for all analyses in this article) closely match the U.S. population in all respects.

**TABLE B. Sample and Population Characteristics**

| | | Full Sample | | Mailback Sample | |
|---|---|---|---|---|---|
| | Population Data | Raw | Weighted | Raw | Weighted |
| Age | | | | | |
| 18 to 34 | 37.0% | 36.4% | 37.3% | 33.6% | 35.9% |
| 35 to 49 | 28.5 | 33.2 | 29.4 | 34.0 | 30.3 |
| 50+ | 34.4 | 30.3 | 33.3 | 32.5 | 33.8 |
| Sex | | | | | |
| Male | 47.9% | 44.8% | 47.8% | 42.2% | 47.8% |
| Female | 52.1 | 55.2 | 52.2 | 57.8 | 52.2 |
| Education | | | | | |
| High school only | 60.2% | 45.0% | 62.0% | 40.4% | 61.3% |
| Some college | 18.4 | 25.7 | 18.3 | 26.0 | 19.2 |
| College graduate | 21.4 | 29.4 | 19.7 | 33.6 | 19.5 |
| Median family income | $35,353 | $36,225 | $35,064 | $37,922 | $36,012 |

*Sources*: Age and sex: U.S. Bureau of the Census 1993, Table 14; Education: U.S. Bureau of the Census 1992, Table 220; Income: U.S. Bureau of the Census 1995, Table 41.

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# History, Historiography, and Political Science: Multiple Historical Records and the Problem of Selection Bias

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*Social scientists who use history as a laboratory for theory development use the work of historians to construct background narratives which can then be coded according to theoretically relevant categories. Yet, virtually no attention has been paid to how these historical monographs are to be chosen. On most periods and themes of interest available accounts differ, not only substantively but also with respect to the implicit theories and conceptual frameworks used to establish salience or produce commonsensical explanations. Unself-conscious use of historical monographs thus easily results in selection bias. Social scientists are bound to be more attracted to and convinced by accounts that accord with the expectations about events contained in the concepts they deploy and the theories they seek to test. Consideration of recent developments in historiographical theory supports the argument that responsible techniques for using historical sources are available, but they require understanding the extent to which patterns within historiography, rather than "History," must be the direct focus of investigation and explanation. Such an approach has the added advantage of helping to generate historically based studies where observations or cases outnumber variables.*

One of the most confounding problems in the use of history as a database for the elaboration and testing of theories is the problem of matching propositions with appropriately controlled comparisons so that in our "natural experiments" cases outnumber variables.[1] This problem may be effectively addressed if we take seriously the fact that our loadings of variables do not spring from a transparent historical record but from a collection of accounts of sets of events constructedly narrated as episodes. In other words, if we treat our database as "historiography" or "histories" and not "History," then the actual number of "cases" expands from the number of episodes to the number of accounts of those episodes. Historiography thus multiplies history, as each pattern in the latter produces the potential for many patterns in the former. Acknowledging our reliance on histories, not "History," will help, not hinder, efforts to find opportunities for study in which cases outnumber variables. More generally, attention to the diversity of accounts from which background narratives must be constructed can help prevent serious theoretical and evidentiary errors, thereby enabling historically minded political scientists and sociologists to defend their enterprise against potentially devastating challenges from both historians and social science methodologists.

This argument claims to identify a silver lining in the cloud that has cast shadows on the exciting work done in the last two decades in historical sociology, comparative history, and historically oriented political science. In these fields, sequence, path dependency, and diachronically arranged focused comparisons of many varieties have been used to develop typologies of political transformation and account for patterns of difference and similarity in the trajectories of political communities separated in time, or in time and space. All these approaches rely on the work of historians for their data, that is, the depiction of past events ready to be coded. The patterns discernible within these depictions are either explained with reference to larger "covering laws" or used to explain other patterns of events—patterns also drawn from the work of historians, or from reports or observations of contemporary experience.

The cloud to which I refer is the largely (now) uncontested claim that the work of historians is not understood by historians to be, and cannot legitimately be treated by others as, an unproblematic background narrative from which theoretically neutral data can be elicited for the framing of problems and the testing of theories. This being the case, an obvious problem arises. How are social scientists, intent on using historical episodes to test their theories or to elaborate their typologies, to choose among differing accounts of the past? If they believe (and implicitly at least most practicing historical sociologists have so believed) that certain accounts are true and others false, certain accounts more accurate and others not, then their theories, and the conclusions they draw from the tests performed on their theories, can only be as good as the rules to which they adhere for distinguishing "accurate" from "inaccurate" historical monographs. Otherwise, their typologies and conclusions could just as well be based on the errors, biases, interpretations, omissions, and exaggerations produced by errant historians, as on reliable reports of past political or social realities.

The problem, of course, is considerably more complex than distinguishing "accurate" from "inaccurate" accounts. In his provocative book on the historiography of medieval Europe, Norman F. Cantor (1991) has described the enormous variability in historians' images of

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[1] For a clear statement of this problem see Fearon (1991, 172). For a treatment which sensibly substitutes "observation" for "case" see King, Keohane, and Verba (1994, 208–30). See also comments on this volume by Caporaso (1995), Collier (1995), and Rogowski (1995) regarding the general problem of having "more explanations than observations."

the Middle Ages and hence the absence of a single "historical record" upon which social scientists could rely for typologies and for testing theories. Cantor's point is not to measure different histories of medieval Europe as better than others (though he does offer many such judgments), or to show how contradictorily different authors treat the same topic (though such demonstrations are also to be found in the book), but to show how each of the twenty major historians whose work he considers presents a picture of "what happened" that is just as much a function of his or her personal commitments, the contemporary political issues with which s/he was engaged, and the methodological choices governing his or her work, as of available source materials. What we know of the Middle Ages, then, is not the result of an objective sifting and reporting of what primary sources and artifacts contain, but the result, first and foremost, of imaginative constructions of lives and events, which, woven into particular overarching narratives, seemed natural, convincing, or useful to these historians as vehicles for the claims they wished to advance (Cantor 1991).

The necessarily interpretive work done by historians thus creates substantial problems for social scientists who may wish to use the medieval period in Europe to test the plausibility of their theories, though obviously the same methodological problem exists in regard to any other historical period or topic.[2] Indeed, for any historically grounded social scientific research, how is the background historical narrative which is to serve as the empirical referent in the investigation to be chosen, discovered, or manufactured? Which sources are to be consulted, which used, which discarded?

## HISTORIOGRAPHY AND THE PROBLEM OF SELECTION BIAS

David Collier (1995, 462) has succinctly described selection bias as "occurring when the *non*random selection of cases results in inferences, based on the resulting sample, that are not statistically representative of the population." Although Collier does not address the question directly, it is clear that whether or not historical accounts used by social scientists to test their theories of human behavior are to be considered as a sample of all possible historical accounts available, or as a sample of "observations" of the one "real" episode that "happened" in the past, the danger of selection bias exists whenever historical sociologists or historically minded political scientists draw upon available monographic material to produce background narratives, "databases," for drawing inferences about their theories.

Consider Theda Skocpol's views on the matter. Skocpol (1984) has recognized the problem of formulating rules for choosing among available historical accounts, though she has not tried to solve it. She rejects as logistically impossible the idea that historically minded social scientists could develop enough expertise as historians in each period and area of interest to conduct their own research on "primary sources" in these areas.

> Redoing primary research for every investigation would be disastrous; it would rule out most comparative-historical research. If a topic is too big for purely primary research *and* if excellent studies by specialists are already available in some profusion—secondary sources are appropriate as the basic source of evidence for a given study (1984, 382).[3]

But how is one to know which studies are "excellent," especially when findings conflict? How, in other words, is the comparative historical sociologist to distinguish objectively valid histories from those that are not? To this question Skocpol responds euphemistically. "Comparative historical sociologists have not so far worked out clear, consensual rules and procedures for the valid use of secondary sources as evidence" (1984, 382).[4] While drawing attention to the variety of "historiographical interpretations" which should not escape the notice of researchers, she proceeds to offer advice in the use of historiography which can easily be seen to aggravate rather than alleviate the main difficulty.

Skocpol comments that since the questions asked by comparative historical sociologists may not be those that historians have asked, the information needed to answer the questions may require the analyst to search for evidence "in minor corners of publications, or in the work of an 'odd' historian out of tune with dominant historiographical trends." Skocpol also encourages researchers in need of evidence not contained in existing monographs to conduct their own "targeted primary investigations," research whose objective would be to organize information into the categories stipulated as relevant by the theory under consideration (1984, 382–3).

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[2] By focusing in this essay on the questions that the selection bias issue raises for social scientists using historically oriented materials, I am by no means suggesting that the problems I highlight have been solved in other areas of social science research. Selection bias is a potential problem in any research endeavor involving general inferences made from particular pieces of evidence or particular data sets. In this article I focus on the problem as it presents itself in qualitatively oriented research using monographic historical studies as the primary evidentiary base. But synchronic studies, comparing dozens or scores of cases (countries, wars, social strata, etc.) along some dimension(s) of interest, are also susceptible to serious problems of selection bias. Such problems are contained but obscured in a strict "stylization" of cases. These accounts are often descriptions in terms consistent with, or even constructed with the aid of, the theory being tested, a theory "corroborated" by the "measurements" thereby made available. Accordingly, I encourage readers to measure the questions I raise and the coping strategies I suggest, not only in their consideration of historically based work but also with respect to any research study in which the author selects from available treatments to form an evidentiary base. Needless to say, I include this very study within that category.

[3] For a contrary view, stressing how important it is for historical sociologists to train and work as historians, with primary sources, see Aminzade (n.d.). Aminzade, however, all but ignores the logistical difficulties such advice presents for carrying out large-scale multicultural, diachronic studies, as well as the epistemological issue of whether a theorist's own construction of the evidence would not be even more likely to present a problem of selection bias than would making the selection from among available monographs.

[4] In a recent influential attempt to specify rules systematically for inference from evidence in qualitative social science, King, Keohane, and Verba (1994) slide over this problem and even contradict their own injunctions when they briefly consider the use of historical sources. While declaring that "*The most important rule for all data collection is to report how the data were created and how we came to possess them*" (p. 51, emphasis in original), the authors make no reference to the problem of choosing among partially conflicting historical accounts. Instead, reference is made to the "historical record" (p. 135) and to "good historians," i.e., those who "understand which events were crucial, and [who] therefore construct accounts that emphasize essentials rather than digressions" (p. 53). How the work of "good historians" is to be distinguished from that of lower quality, i.e., how the "data collection" is to be accomplished, is treated, implicitly, as if it were methodologically unproblematic.

Note that this advice is wholly focused on gaining access to needed information, that is, reports about events that are not expected to require further defense or corroboration and are not themselves understood as embedded within, or actually reflections of, theoretical commitments. In other words, searching for "facts" which may be necessary to test a theory of interest to an historically minded social scientist may mean, as Skocpol indicates, searching high and low for monographic work containing the kinds of information and narratives needed, organized in roughly the necessary ways, or even constructing the work oneself, with one's own theory as a guide for data collection. But here is the nub of the issue. Such a search may well entail, and can logically be supposed to entail, a heavy selection bias toward works by historians using implicit theories about how events unfold and how people behave very similar to the theory under consideration by the social scientist. Indeed, in a recent discussion of her methodology Skocpol is admirably explicit about the influence her theories exerted on the design of her background narratives.

> When I think back on how I developed the narrative parts of this book [*States and Social Revolutions*], I realize just how profoundly informed those narratives were by the concomitant comparative-causal analyses. . . . narratives do not just spring forth naively. It was precisely the comparative analyses in SSR that made it possible for me to write the narratives of what happened during the revolutionary breakdowns and the reconstructions of the old regimes into the revolutionized new regimes in France, Russia, and China (1995, 44).

Consider as well the case of Aristide Zolberg's (1980) article tracing modern state consolidation in what became Great Britain and France to patterns of strategic interaction among a multiplicity of principalities, "territorial aggregates," and "monarchical states." Zolberg's argument is that the general character of the European state system, many key features of the internal structure of European states, and the particular size and shape of Great Britain and France are basically functions of the juxtaposition of violence prone rivals and the strategic imperatives under which they operated as a result of this juxtaposition.[5] My interest here is not in how valid the argument is but in the way Zolberg produces the background narrative of late medieval and early modern European history with which to test his theory. For the sixteenth and seventeenth centuries, which form the most important period for his argument, he reports that he "shall largely rely" on Ludwig Dehio—*The Precarious Balance*. In the note appended to this sentence Zolberg explains his choice.

> Unless otherwise stated, our discussion of the formation of the interstate system at the beginning of the modern era will be based on this unjustly neglected classic (published in 1948 under the title *Gleich-gewicht oder Hegemonie*) (1980, 698 and 715n).

Whether Dehio's work has or has not been unjustly neglected is less interesting here than the fact that his account of European history in the sixteenth and seventeenth centuries is *entirely* framed as a story of realpolitik—a succession of clashes and strategic interactions among hard-shelled states whose foreign policies and military capacities are virtually their only important features. Dehio's claim is that the same "fertile friction" which produced excellence among the competing Greek city states, produced extraordinary and characteristic development of national states in Europe—"an immense heightening of all vital energies." Dehio is admirably explicit about the conceptual and theoretical parochialness of his account. Indeed, he warns his readers that they may develop a distorted view of European history if they base it solely on his book.

> In discussing the history of the system of states, I propose to follow in Ranke's footsteps. . . . the state remains for us, as it was for him, the central factor in history: the point at which history's main lines of development intersect and its great impulses converge, the chief influence shaping our existence by virtue of order and power. This is the way we Germans have experienced the state; and by the same token, it is our experience that in times of great crisis the state's will to live operates most forcefully in the field of foreign affairs. . . .
>
> In adopting this method, I am conscious of adhering to a fundamental tradition in the German approach to history. The destinies of other countries may demonstrate the effectiveness of other approaches. It will suffice if we find a common meeting ground with them at the end of our study (1962, 5–6).

These conceptual and (implicitly) theoretical commitments form the basis of Dehio's sweeping narrative, showing how the Hundred Years' War between England and France consolidated the former and produced an exceedingly powerful France. France's strength was then expressed by its thrust in the late fifteenth century toward conquest and absorption of Italy. Checked in Italy by the Habsburgs in Austria and Spain, France allied with the Ottoman Empire to weaken the Habsburgs and prevent a new Holy Roman Empire from emerging under Charles V. The story of thrust and counterthrust, hegemonic ambition and counterbalancing alliances, continued through Phillip II, Louis XIV, and Napoleon, with Britain emerging as a powerful balancer. Then, Germany finally managed to achieve unity and take center stage in the twentieth century, initiating wars whose scope ultimately submerged the European continent within a global system dominated by the United States and the Soviet Union.

What is crucial to notice, of course, is how exactly Dehio's conceptual apparatus and theoretical commitments correspond to those which Zolberg is seeking to test (or corroborate) by using Dehio's account as a "theoretically neutral" background narrative. By relying on Dehio's account and by not discussing its distinctiveness, Zolberg is running the serious risk of selection bias on the dependent variable. An ungenerous characterization of the article would be that it advances a tautological argument whose conclusions are derived from sources chosen out of those available as a function of the conclusions themselves.

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[5] This is an argument, echoing Otto Hintze (1975), which has also been made by Samuel Finer (1974, 1975), Bruce D. Porter (1994), Charles Tilly (1985), and others.

In general terms the problem highlighted by the Zolberg article may be stated as follows. Suppose a researcher is advancing claims about typology "T" and theory "H" and intends to test their applicability or explanatory/predictive capacity against particular historical episodes. My contention is that it is precisely those historians' accounts framed according to categories and propositions that match typology T and theory H which the researcher is likely to find most useful as the (supposedly) theoretically neutral factual or historical record on the basis of which T and H are to be tested—even if, in Skocpol's terms, these accounts represent the work of "odd" researchers. Skocpol implies that the kind of "odd" work to which she refers is of high quality but "out of tune with dominant historiographical trends." So it may be. But it may also be unreliable work which is so convenient that questions about its reliability and its historiographical "oddity" may be ignored.

The primary methodological problem is thus not, as Skocpol suggests, the difficulty of finding necessary information. The more daunting question is how to choose sources of data without permitting correspondence between the categories and implicit theoretical postulates used in the chosen sources to ensure positive answers to the questions being asked about the data. For social scientists this type of problem—unintended contamination of observations due to the analyst's disposition to corroborate hypotheses—is not an unfamiliar problem. When a psychologist knows what experimental condition or which category of treatment applies to individual subjects, his or her observations of behaviors are likely to be biased toward hypothesis corroboration. Social psychologists therefore take elaborate precautions, including double-blind designs, to compensate for this phenomenon. If the shelves of a library can be imagined as equivalent to subjects in such an experiment, the historically minded social scientist examining books and journals on those shelves is in a position comparable to the psychologist observing subjects. If the selection of sources is performed unself-consciously and without adequate safeguards, then one must presume that there will be a tendency to elicit from the shelves of the library, as most reliable and relevant for constructing the "background narrative," those sources organized most consistently with the terms of the researcher's argument. Theories may therefore likely be corroborated even though the best or the bulk of relevant work by historians would not support, or even stand as consistent with, the theory under consideration.

## "HISTORICAL FACTS" AND THE CASE OF BARRINGTON MOORE

The problem I have adumbrated has not been ignored completely, but the ratio of heat to light generated by at least one prominent discussion of the matter reflects poorly on the seriousness of attempts that have so far been made to address it. In an article published in the *British Journal of Sociology* in June 1991, John H. Goldthorpe set off a small storm of criticism by including within his discussion of "recent tendencies" in historical sociology the adoption of what he called an "essentially positivistic view of historiography." By this he meant that practioners of comparative historical sociology often treat the work of historians as sources of "facts" instead of as interpretations of materials which produce claims that are then susceptible to being treated as "facts" by social scientists eager for a convenient database (1991, 220–2). This allows historical sociologists, as Goldthorpe puts it, "to enjoy a delightful freedom to play 'pick-and-mix' in history's sweetshop" (p. 225).

Goldthorpe cites Skocpol's awareness of the problem but questions, as I do, her advice for coping with it. He proceeds to illustrate his point by analyzing what by all accounts would seem to be the single most influential and highly regarded book of its genre, Barrington Moore's *Social Origins of Dictatorship and Democracy* (1966). Goldthorpe reminds his readers that Moore's treatment of the English case relied on an image of the English Civil War as a challenge to the old order sustained by "commercially minded elements among the landed upper classes, and to a lesser extent among the yeomen." As reflected in Moore's citations, the "factual" basis for this claim is of R. H. Tawney's *Agrarian Problems of the Sixteenth Century* (1912); Tawney's essay on "The Rise of the Gentry" (1941); and Mildred Campbell's *The English Yeoman* (1942). The claim, advanced as factual, that the English Civil War was in essence a bourgeois revolution is a crucial part of the background narrative, or database, that Moore uses to illustrate the elegance of his typology of trajectories into the modern world and the power of his theory identifying revolutionary violence in the transition from feudalism as a prerequisite for democracy. As Goldthorpe points out, however, this interpretation of the English Civil War, although it coincided spectacularly with Moore's argument, is but one interpretation of events in England in the seventeenth century; was actually falling out of favor with historians at the time Moore chose it as the record of events which his theory would have to explain; and ignores or severely denigrates the role of religious belief as central to the conflict (1991, 222–3).

It is worth going into somewhat more detail than did Goldthorpe in his analysis of Moore. In order to portray the English Civil War as a bourgeois revolution, Moore required a bourgeois class to be in existence by at least the second quarter of the seventeenth century. For this history Moore relies, heavily and explicitly, on what he refers to as "R. H. Tawney's unsurpassed study of the economic life of England before the Civil War" (1966, 6). To establish Tawney's study as "unsurpassed," Moore provides two lengthy footnotes defending Tawney's theory (described as a "fact") against criticisms launched by other historians—a theory of early adaptation and inclusion by landed English aristocrats of commercial elements and orientations (pp. 6–8). Such awareness of historiographical diversity is praiseworthy and highlights the analytic importance of Moore's choice of sources. But in this context it is also significant, if not decisive, to note how closely aligned are Moore's ideas with the theoretical foundations of Tawney's work as well as Cantor's judgment that claims about the early formation of commercially minded middle-class solidarity in England are "wrong." According to Cantor, "the social amalgamation of a universal middle class was in fact still going on in the middle decades of the nineteenth century." Cantor points out that "in order to turn the great rebellion of the 1640s into a bourgeois revolution, Tawney had to get the gentry and burghers molded into one revolutionary class in the sixteenth century" (1991, 389–90). Similarly, for the English case to corroborate his theory, Moore required Tawney's work to be "unsurpassed" by other historical views.

The point can be made even stronger. One of Moore's self-proclaimed objectives was to debunk the "elements of myth in common notions about the peculiar British capacity to settle their political and economic differences through peaceable, fair, and democratic processes" (1966, 4). For this reason as well he needed the civil war in the seventeenth century to be fought between socioeconomic classes over economic issues. For that to be the case he needed interpretations of English history which placed the crystallization of the English commercial class before the violence of the seventeenth century and not during or following the peaceful eighteenth and nineteenth centuries. This particular "emplotted" (White 1973, 6–7) sequencing of events (i.e., this particular narrative) was therefore vital if Moore was to show England as corroborating his theory: not only establishing materialist motives and socioeconomic class as the focus and unit of analysis but also showing that, even in supposedly peaceable England, violent, revolutionary transition was a necessary basis for establishing capitalist democracy. Remember, however, that Moore put forward the interpretation shared by Tawney and Campbell as the factual basis for his depiction of the English case, a *specific* and highly parochial interpretation that was based on the same conceptual categories and theoretical predispositions that Moore was seeking to test by using the interpretation not as theoretical but as "factual."[6]

Moore's argument about England is actually considerably more convoluted than this. Trying to summarize his position, without exposing himself to contradiction by those who would cite the embryonic status of class formation in the seventeenth century and the massive enclosures of the two following centuries as more decisive in the crystallization of class structure than the Civil War, Moore writes that

> there would seem to be little reason to question the thesis that commercially minded elements among the landed upper classes, and to a lesser extent among the yeomen, were *among* the main forces opposing the King and royal attempts to preserve the old order, and therefore *an* important cause, *though not the only one*, that produced the Civil War (p. 14, emphasis added).

In the conclusion of the book Moore in fact virtually abandons his claim that economic motives were important during the English Civil War. Recasting his argument to emphasize the consequences of the struggle and not its content, Moore shifts the term "bourgeois revolution" to refer to violent transitions which benefit the bourgeoisie as an unintended consequence, even when the revolution was carried out by people who could not be considered or did not consider themselves bourgeois. Meanwhile, "peasant revolutions," as he uses the term, refer to violent transitions which are carried out by and for the peasants. Admitting his inconsistency, Moore makes this move so that, at least on the rhetorical level, he can portray the English Civil War as the kind of struggle and class realignment his theory of revolution, class struggle, and democracy requires (1966, 428–9).[7]

In sum it is probable, if not certain, that selection bias, not discovery of the single correct historical depiction, lies beneath Moore's rhetorically convincing and theoretically satisfying use of the English case to advance key portions of his argument.

To the extent that historical sociologists, apart from Skocpol, are aware of this problem, how have they responded to it? Their reaction to Goldthorpe's article is a convenient way to answer this question. One of the sociologists whose work Goldthorpe criticized, Michael Mann, was one of four sociologists whose responses were published in a subsequent issue of the *British Journal of Sociology*. But in his comment, and in those of the other three respondents, the main line of attack was to attribute to Goldthorpe (and then vehemently reject) arguments he did not make, namely, that historians deal only with facts and not with theories, that historical sociology should be abandoned as a scholarly enterprise, and that Barrington Moore's form of historical sociology is no more sophisticated than that of "the Spencerian/Durkheimian/Parsonian oriented modernization theorists" (Hart 1994, 28–9; Mann 1994, 40–1; Mouzelis 1994, 32).

Insofar as Mann and the other respondents did try to grapple with Goldthorpe's main contention, that monographic studies of historical episodes contain theoretical claims and thus cannot be used unself-consciously without selection bias, their efforts were even less satisfactory than Skocpol's. Nicky Hart dismissed Goldthorpe's concerns as "ludicrous," contending that sociologists criticize one another's work vigorously, indeed even "ungenerously," implying that this criticism induces care in the judgment exercised by historical sociologists. But the emphasis of her response belies the role she attributes to vigorous challenge in an open market of evidence and ideas. Without responding to any of the specific criticisms made against Barrington Moore's treatment of the English case, she proceeds to launch an ad hominem attack on Goldthorpe for being insufficiently deferential!

> Why does Goldthorpe feel impelled to warn others of the dangers of historical sociology? Does he believe that he has some privileged capacity to judge the merits of Barrington Moore's use of evidence that others lack? Furthermore, even if some part, even the bulk of *The Origins of Dictatorship and Democracy* were judged to be inadequate on

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[6] For a rather thorough consideration of Moore's account in light of recent historical and geographical scholarship, see Goldstone (1991, 64–83).

[7] For a critical discussion of the place of this kind of argumentation within the spectrum of available approaches to the English Civil War, see Dray (1984).

> account of its empirical proofs, who, other than Goldthorpe, would want to say that sociology or history would have been better served if the book had not been published in the first place (1994, 28)?[8]

Nicos Mouzelis's argument against Goldthorpe is similar to Hart's in his reliance on an appeal to Moore's reputation and his apology for any errors Moore may have made in his depiction of the English Civil War as but "a moment in the ongoing debate" (1994, 31–4). Acknowledging (if only by implication) that Moore may have distorted the English case by adopting sources that suited his theory, Mouzelis condemns Goldthorpe for not praising Moore as an improvement over previous generations of historical context-insensitive sociologists such as Auguste Comte (1974); David McLeland (1961); Neil Smelser (1959); and Herbert Spencer (1904, 1911).

Mann's own response to Goldthorpe is breathtakingly off-point. While acknowledging the correctness of Goldthorpe's specific criticism of Moore's use of sources in the English case ("Moore did not fully prove his case."), he avoids meeting the methodological issue Goldthorpe was raising by his focus on this aspect of Moore's book. Indeed, Mann denies even understanding what general point Goldthorpe's "attack" might have.[9] Instead, as did Hart and Mouzelis, Mann construes Goldthorpe's critique of Moore and his insufficient recognition of Moore's contribution as an exercise of insufferable temerity.

> The grandeur of (Moore's) enterprise and the importance of his conclusions ... resonated among a whole generation of sociologists and historians. His conclusions provided testable propositions about the preconditions of democracy. Some of the most important have stood up well to a battery of subsequent testing. Of how many works of 1966 sociology could one say that? Are there any? (1994, 46).
>
> For a prominent European sociologist [Goldthorpe] to advocate such a massive separation between the practices of history and sociology *in 1991* is as bizarre as it is appalling. It is important that sociologists, especially current sociology students, do not take him seriously (p. 51).

Mann claims that he "fail[s] to understand" Goldthorpe's concern with the methodological question of how historical sociologists can go about choosing among contradictory, or just different, secondary sources. When he becomes sceptical about "historians' facts," Mann notes, there are "other sources of information." What are they? The answer is stunning in its confirmation of the experimenter-bias-toward-corroboration dynamic I have argued can easily afflict the historically minded social scientist. "If I am sceptical about historians' facts, I have other sources of information (*my theories based on broader knowledge about how societies operate*, and my knowledge of historians' theories)" [1994, 41 (emphasis added)].[10] This, of course, is precisely the problem. If social scientists test their theories against historical episodes by selecting those accounts of the episodes which are organized and presented according to the categories and propositions of the theories they are testing, then it will be no great surprise to learn that the theories receive "empirical" support from the exercise.

In his reply to Goldthorpe, Joseph M. Bryant (1994) recognizes the force of Goldthorpe's methodological critique, admits that "fixed rules" (p. 3) for conducting a triage of secondary sources do not exist, and hopes that "Goldthorpe's pointed challenge will elicit fuller disclosure of analytical procedures from practicing historical sociologists in the future" (p. 13). But in offering his own solution to the problem, Bryant can do no better than allude to his faith in the way historical sociologists now use "theory and comparative evidence" (p. 14). Praising the vigor with which comparative historical sociologists criticize one another's work, he assures us that "general scientific standards or criteria do currently function to determine the respective strengths and inadequacies of published research" (p. 14).[11]

As unsatisfying as these responses to Goldthorpe may be, they are correct insofar as they wonder whether Goldthorpe has himself offered any solution to the problem he has identified. Indeed, he has not. It is precisely for this reason, one could speculate, that responses to his argument have been so defensive and so nearly, in some cases, hysterical. For without an intellectually satisfying response to the problem of selection bias in historical sociology, the whole field is vulnerable. As Skocpol and others recognize, grand historical sociologists, or even not-so-grand comparative historical sociologists (or political scientists), cannot afford to become top-flight specialists in the history of each episode and its context that they will need to use for arranging natural experiments.[12] Absent a solution to the problem of theoretically weighted and contradictory historiography, therefore, it may be politically wise, or at least irresistible, to deny the problem exists at all.

## HISTORIOGRAPHY AS THE REFERENT FOR THEORY

As with so many other problems that have arisen for twentieth-century social scientists, one's first impulse might reasonably be to consider how Max Weber dealt with selection bias in his treatment of historical evidence. After all, Weber was primarily an historian—an historian whose nomothetic tastes led him to investigate the past by developing abstract categories and typologies for identifying generalizable and recurring patterns.

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[8] Goldthorpe, to be clear, neither claims that the academic world would have been better off without Barrington Moore's book nor, as he explains in his response to his critics, proposes an "end to grand historical sociology" (1994, 69). For an influential recent treatment that relies heavily on a kind of "free market" mechanism for arriving at truth via arguments among informed historians with access to primary evidentiary materials, see Appleby, Hunt, and Jacob (1994).

[9] "What is its *point*? What is the *general* significance of attacking Moore for selective use of sources on the English Civil War? There is none ..." (Mann 1994, 46).

[10] Mann's mention of "my knowledge of historians' theories" as a resource in the evaluation of available historiography contains the beginnings of a positive response to Goldthorpe, but it is undeveloped.

[11] Oddly, it is precisely Skocpol's essay in *Vision and Method*, referred to earlier for its recognition of but failure to address the selection bias problem, that Bryant provides as his sole illustration of why we should have faith in the methodological practices which prevail among contemporary historical sociologists (1994, 14n).

[12] See footnote 2.

Weber (1949) also paid careful attention to methodological problems and to various aspects of the problem of "objectivity." Indeed, Weber's life work can be understood as an historian's struggle to make his work scientific, not so much by adopting the formal pose of an "objective" observer weighing available evidence in the interest of truth but by integrating the constitutive role of theory and value, which Weber believed existed in the natural sciences, into the practice of history writing. Thus, sociology would be the vehicle for historical explanations of concrete events—a science distinguished from the natural sciences mainly by the possibility of achieving empathic understanding of the meaning of events for participants. For my purposes, what is important to note is that Weber's consideration of "objectivity" did *not* lead him to address the problems of evidence gathering faced by social scientists who would use the work of historians. Nor did he produce a satisfying solution to the more general problem of selection involved in scientific research, though he did emphasize that a priori selection of objects of study from the infinitely complex mass of reality was an issue that confronted both the natural and "cultural" sciences (Burger 1987, 201–27 and 269n; Goddard 1973; Iggers 1983, 159–68; McLemore 1984).

If a solution to the problem of selection bias in the use of historical materials cannot be found in Weber, the beginnings of a solution may be found in recent trends within historiographical theory. The contemporary discipline of history as a profession can be traced to the attempt by the great German historian of the early nineteenth century, Leopold Von Ranke, to establish the work of historians as a scientific activity (Iggers 1984). Ranke's enormous influence over German and then most other Western historians, from the mid-nineteenth century until the interwar period, was based on promoting a "scientific" method for the vetting and analysis of documents. It was claimed that with proper training in the analysis of documents and the use of official archives historians could distinguish the truth of what had actually happened in the past from false beliefs about what could have happened but did not.

But the scope of Rankean history was exceedingly narrow. Its obsession with meticulous treatment of documentary evidence helped confine its practitioners to the study of high politics, especially diplomacy. Its commitment to render historical material into narratives that fit with documentary evidence of specific episodes gave little scope for the generalizations, patterns, and structural effects that historians with an increasing interest in sociology sought to advance and explore. Dissatisfaction with Rankean methods and subject matter developed slowly in the late nineteenth century, although it was reflected in both Weberian and Marxian approaches. The most direct and influential reaction against Rankean historiography was made by a new breed of "social historians" led by French scholars associated with *Annales d'Histoire Economique et Sociale* (the "Annalist" school). Social historians challenged the Rankean presumption that all events of significance were recorded in the archives of established states and that high politics and diplomacy were the only things of real interest in the "historical record." Instead, they delved into the history of ordinary life as lived by the masses. They collected data from marriage registers, tax rolls, account books, and other indexes of mundane affairs, integrated economic and geographical materials into their work, and described the *mentalités* characteristic of different times and places. While prizing the scientific attitude toward data emphasized by the Rankeans, Annalists eschewed periodizations set forth in official histories of states, preferring to ask their own questions of the evidence they gathered and let appropriate periodizations emerge from the problems they sought to solve from this evidence. Their aim was to describe broad regularities, trends, and structural effects rather than precisely to evaluate official documents for the purpose of describing specific sequences of events accurately (Burke 1992, Dosse 1994).

While the social historians were moving historians away from politics, toward an interdisciplinary mixture of sociology, anthropology, and economics, the Rankean commitment to finding a scientific or at least systematic understanding of historical writing as a distinctive form of inquiry remained. In the interwar period R. G. Collingwood (1994) argued that the task of the historian was to find and interpret evidence so that the mental state of historical actors could be discerned. Via a narrative form, the past could be "mentally reenacted." Without claiming that any one historian's narrative could ever be known to be the absolute truth about the past, Collingwood nonetheless held that truth to be the goal and standard of the work of historians as they seek more verisimilitude and completeness in the reenactments that their narratives make possible. This approach understood historians to be working, in a broad sense, according to the canons of "scientific thinking" but denied that history was, could, or should be understood as a social or natural science (1994, 217–28; 269–70; 390–2).

The question remained: Of what were these narratives composed? Most historians seemed to imagine they were based upon collections of individual facts ascertained as such by careful inferences from the "relics" of the past. Little attention was given to the role of implicit theories about human behavior that served as the "common sense" source of these inferences from the relics. This stance, rejecting the utility of theory when confronted with the complexity of human behavior, yet using theory constantly via inferences employed in the construction of historical scenarios, has often been a source of frustration for many social scientists in their interaction with historians.

Yet, even in the 1920s some historians were beginning to see the problem as social scientists saw it.[13] This shift was reflected subsequently in the virtual canonization of a 1926 paper by Carl Becker, "What Are Historical Facts?" As Harry Elmer Barnes (1937) put it, Becker "exploded once and for all [this] basic illusion regarding historical facts. He exposed relentlessly the fallacy involved when we talk about the 'hard facts' or the 'cold facts' of history." Barnes proceeded to quote Becker directly.

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[13] Indeed, one of the most instructive aspects of the brouhaha over the Goldthorpe article is that Goldthorpe's argument against practices by sociologists (treating "historical facts" as positive, atomistic, nontheorized building blocks of reality) is *precisely* the argument that social scientists made against the practices and beliefs of historians in the nineteenth and early twentieth centuries. I am referring to efforts to convince historians, who dismissed models, hypotheses, and theories as irrelevant to their task of ascertaining and reporting "the facts" about

> By virtue of talking in this way, the facts of history come in the end to seem like something solid, something substantial like physical matter, something possessing definite shape and clear, persistent outline—like bricks and scantlings; so that we can easily picture the historian as he stumbles about in the past, stubbing his toe on the hard facts if he doesn't watch out (p. 266).

Becker illustrated his point by showing how the "fact" that Caesar crossed the Rubicon did not, per se, exist. "A thousand and one lesser 'facts' went to make up the one simple fact that Caesar crossed the Rubicon; and if we had some one, say James Joyce, to know and relate all these facts, it would require a book of 794 pages (i.e., *Ulysses*) to present this one fact that Caesar crossed the Rubicon" (Barnes 1937, 266–7). The so-called simple historical fact, argued Becker, "is, so far as we can know it, only a symbol, a simple statement which is a generalization of a thousand and one simpler facts which we do not for the moment care to use and this generalization itself we cannot use apart from the wider facts and generalizations which it symbolizes" (p. 268).[14]

By the end of World War II this view was strong enough, especially among U.S. historians, that the profession faced a kind of crisis in its disciplinary identity. Narrativist approaches which ignored or denied the role of generalizations, concepts, models, and theories in the construction of evidence and in its interpretation could not satisfy the aspirations of many historians to understand their work scientifically. In reaction, many historians turned toward the idea of history as itself a social science, or even a kind of synthesis of the social sciences. Previously this scientific or scientistic urge had found expression in Ranke's emphasis on the technique of documentary evaluation and analysis, strictly confined to the archives of states, or in the vast Hegelian, Marxian, Spenglerian, or Toynbee-style efforts to discover the Laws of History. Now, however, the ambition to treat history scientifically entailed adoption by historians of the methods and analytic vocabulary of social science. This view found its most representative expression in the writings of Louis Gottschalk (1963) and the scholars who contributed to the Social Science Research Council's reports on this topic in the 1940s, 1950s, and early 1960s and of Geoffrey Barraclough (writing for UNESCO) in 1979.[15]

For these historians, becoming scientific meant abandoning "narration" as the special technique of their profession; abandoning, in other words, emplotted interpretations of chronologically ordered episodes which could be believed as "that which did happen." Instead, historians would have to become self-conscious and scrupulous about the assumptions and generalizations used to frame their arguments. Barraclough argued that historians could survive as respectable scholars only by becoming scientists, by searching for Hempelian "covering laws"—abstract claims about classes of events based on the best available theories produced by sociologists, economists, and political scientists (Hempel 1968). Like Ranke, the historians-as-social-scientists would advance their work as scientific. Like Ranke, they would ultimately claim to produce accounts of what had actually happened in the past. But unlike Ranke, they would produce accounts not concerned solely with the behavior of states and their leaders. Nor would they rely only, or even mainly, on official archives for the uncovering of evidence.

But History as a social science has not fared well. As consumers of social theory produced in other disciplines, historians could do little more than retell the stories they were producing in the vocabularies of (usually) outdated, cobbled together theories from sociology, anthropology, psychology, and political science (Burke 1992, 19–21). While training in statistical methods and computer technology did encourage many historians, especially in the United States, to pursue aggregate data-based research (cliometrics, as it is called, practiced by many members of the Social Science History Association), the profession as a whole was unsatisfied with scholarly norms which recognized no intellectual comparative advantage attached to the work of historians, per se.

The epistemological and methodological malaise felt by many historians in the 1970s was alleviated with what Lawrence Stone (1979) called the "revival of the narrative." Stone's treatment emphasizes the failings of the various "scientific" historical approaches and the market for narrative history which provided publishing and economic opportunities for scholars ready to tell interesting stories. Although Stone judged the new narrativist turn in historiography to be methodologically superior to the old Rankean model, it is in the work of scholars such as Louis O. Mink (1987a, 1987b, 1987c) and Hayden White (1973, 1987) that one finds a fresh and extremely useful understanding of the explanatory function of narrativity.[16]

Mink's conception of historical work builds on argu-

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what actually happened in the past, that no claims about the past could be made *without* the use of general concepts and implicit theories.

[14] Quoted in Barnes (1937, 266–8); see also Mink (1987c, 94–5).

[15] See Barraclough (1979), Gottschalk (1963), *The Social Sciences in Historical Study: A Report of the Committee on Historiography* (1954), and *Theory and Practice in Historical Study* (1946).

[16] The narrativist position associated here with Stone, Mink, White, and others adumbrated below stands between two extremes not discussed here. One, associated with such neo-Rankeans as G. R. Elton (1967), Leon J. Goldstein (1976), and Maurice Mandelbaum (1938), advances what can be thought of as the Forrest Gump theory of history: "History is as historians do." They assert that when skilled historians apply their training well they do discover the facts of the past, which stand outside any optionally applied narrative schema. The other extreme, exemplified by Frank Ankersmit, takes a strong postmodernist position that historical writing is an exercise in memory and an opportunity for playful and perhaps productive reflection on the way the past can be remembered, with no claim to be discussing the actualities of the past or moving toward an accumulation of data usable by social scientists. See Ankersmit (1989). Neither of these two approaches opens up opportunities for the responsible use of historical writing by social scientists.

ments by Arthur Danto (1985) and William Dray (1984) that narrative is a particular kind of schema, one which configures stipulated events into an order that corresponds to a story (with a beginning, middle, and other attributes of a story). Explanation, logically equivalent to that produced by applying Hempelian-style covering laws (Hempel 1968), can be achieved as long as expectations generated by prior acceptance of the abstract narrative schema (that the events to be arrayed are arrayed in an emplotted, narrative, arrangement) are met. Despite the formal similarities of historical method to fiction, the claim made, and supported, by good historical research and writing is that a particular narrative, present in the past, has been discovered, not invented. According to Mink, this kind of truth is the reward of historians who do not, as social scientists do, produce "detachable" (abstract) conclusions—propositions about classes of events that apply beyond their configured discovery of the past.[17]

Hayden White's influential work goes beyond this argument. While Mink envisions historians emplotting their material according to a wide-ranging and unsystematic array of possible stories, White suggests that narratives available to historians, and applied by them, fall within the categories of his typologies of tropes (variations of Comedy, Satire, Tragedy, and Romance). Reports about and relics from the past, what White calls "the unprocessed historical record" (1973, 5), are organized by the historian within a story falling within one or the other of these categories. Thus, occurrences and the patterns they form are, in effect, explained by noting that they could (roughly) be predicted from the type of narrative within which they are asserted to have been implicated.[18]

A major issue of debate among contemporary historiographical theorists is the difficulty of maintaining a position that is "constructivist" enough to recognize the unavoidable intrusion of point of view, implicit theory, and interpretive tropes in the production of "history," but "realist" enough to ascribe actual truth value to some historical accounts. White himself has not accomplished this feat, though he has committed himself to it.[19] But it is not White's particular typology which is of value here for solving the selection bias problem in historical sociology and for moving toward an epistemologically valid and methodologically feasible way to integrate constructivist and realist principles in the work of both historians and social scientists. Rather, I wish to draw attention to the general and vigorous assertion by historiographical theorists, such as Danto, Dray, Mink, and White, and practicing historians such as Cantor and Stone, that the work of historians is actually and respectably comprised of the telling of stories.[20] These stories establish relational meanings among arrays of relics and reports (from the past) while reflecting the biases, common sense theories, general propositions, and political interests of the authors. This means that historiography available for exploitation by historical sociologists, or historically minded political scientists, is best understood as a distribution of hybridic theoretical hunches about what did really happen in the past. Accordingly, considering both the theory and practice of historians as scholars, variation among these accounts must be understood to reflect three different kinds of factors:

- the way that the past actually unfolded;
- variation in the way relics of the past have been stylized by the institutions that produced them so as to ensure their survival and make them available as "primary sources" for historians; and
- variation in the implicit theories, narrative tropes, and political and personal interests of the historians.

Thus, to the extent social scientists use the work of historians (historiography) as a vehicle for access to the past, it is not access to the variety of possible behaviors and patterns that historiography offers, but access to the variety of behaviors and patterns that historians, governed by their own implicit theories, commitments, and access to evidence, have identified. To be sure, just as clever strategies for operationalizing variables can enhance prospects for evaluating general claims in whatever domain they may be applied, so, too, in historically minded social science is there substantial room for methodological maneuver. What is required, though, is recognition that background narratives are constructed, not discovered, and that they carry theoretical and perspectival commitments which raise significant problems for, and can invalidate, the use of history as a laboratory for social science.

## IMPLICATIONS FOR THE PRACTICE OF HISTORICALLY ORIENTED SOCIAL SCIENCE

This essay has sought to draw attention to the difficulties, for social science consumers of historical monographs, associated with the unavailability in those monographs of a transparently true and theoretically neutral historical record. Its purpose is neither to claim that all historical accounts be treated as equally untrue or incomplete, nor to suggest a recipe of "clear, consensual rules and procedures" (Skocpol 1984, 382) for winnowing accurate from inaccurate historical monographs. First of all I doubt that such a list of rules is possible. But it is not the inaccuracy of some accounts or the unavailability of some information which poses the greatest methodological challenges (since no single absolutely accurate and fully informed historical record exists), but rather variation in the implicit theories and typological categories that lie behind these accounts—variation which can lead to selection bias toward sources organized more consistently with the theories under test than other sources. My main objective, then, is to encourage historically minded sociologists and political scientists to direct explicit and systematic attention to historiography and to demonstrate self-consciousness in the selection of source material and in the construction of stylized background narratives.

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[17] See especially Mink (1987a, b, c). See also Danto (1985). Concerning W. H. Dray's ideas and influence, see Shapiro and Sica (1984).

[18] Although reluctant to make this claim explicitly, it is difficult to read White without attributing this argument to him. See especially (1973, 12): "I might want to say that, insofar as a historian provides the 'plot' by which the events in the story he tells are given some kind of formal coherence, he is doing the same kind of thing a scientist does when he identifies the elements of the nomological-deductive argument in which his explanation has to be cast." See also (1987, 31, 43–4, and 219–20.

[19] For a detailed account of White's project and its limits in this regard, see Kansteiner (1993). For serious attempts to square the circle of discovering the "real" facts of history versus constructing working narratives that incorporate interpretations of the available "relics," see Hobart (1989), Martin (1993), Norman (1991), and Van Den Braembussche (1989).

[20] For general sketches of the development of the discipline that detail the context and logic of this narrativist turn, see Iggers (1984) and Bann (1981).

I fully recognize that the problems I have discussed pale in comparison to the paralysis that could beset social scientists interested in history if the work of historians were to be viewed as no more than a collection of preferred or idiosyncratically produced narratives with no useful relationship to past events—events conceived as having really occurred as opposed to other conceivable events which did *not* occur. I will therefore conclude by discussing strategies for using the work of historians without adopting an epistemologically vulgar attitude toward sources of historical information.

In my work in comparative political history, *State-Building Failure in British Ireland and French Algeria* (1985) and *Unsettled States, Disputed Lands: Britain and Ireland, France and Algeria, Israel and the West Bank-Gaza* (1993), I found myself confronted with the issues I have raised in this essay. In some respects I believe the first book suffers from the kind of selection bias phenomenon I have outlined here. In the more recent work I sought to avoid these problems by using historical materials as sources for presenting background narratives *and* as protagonists in a variety of historiographical debates pertaining to each of the long-term relationships under study. An example of how I dealt with variation in historiography, while using it as the basis of my background narrative, is my treatment of William Gladstone's famous "conversion" to Irish Home Rule in 1885.

In the context of my argument, Gladstone's embrace of Irish nationalist demands for autonomy and, eventually, independence from Britain is a crucial turn of events. I take it as a display of political entrepreneurship on the part of the prime minister and leader of the Liberal Party that fulfills one of the three conditions I stipulate for the waging of a successful "war of position"—in this case against the ideological hegemony of the belief that Ireland formed an integral part of the United Kingdom. The historiography on Gladstone and on the Irish question in British history is vast. Aside from older biographies celebrating Gladstone as the "Grand Old Man" and the greatest statesman of his age, I identified half a dozen or so more recent monographs that deal quite contradictorily, but in considerable detail, with the reasons (error, or self-interest, they say) for Gladstone's sudden public declaration of his position on Ireland. He declared for Home Rule, these sources argue,

> because he construed it as a great and terrible issue whose threat to the nation could only be met by his special talents, and with which he could rationalize his reluctance to retire from politics (Jenkins 1988); because his own brand of liberal imperialism required its extension to Ireland as to England's other colonies (Dunne 1975, 435); because of an "eccentric historicism" and because he was panicked into an unreasoning fear of Irish social revolution by the exaggerated reports of key advisers (Loughlin 1986,); because by 1886 an alliance with Parnell was the only way he could form a government (Kee 1982, 131; Marsh 1978, 84); because he could thereby purge the Liberal Party of Radicals and Whigs who opposed his leadership (Cooke and Vincent 1974); or, somewhat contrarily, because he saw Home Rule as a great unifying theme for a Liberal Party divided and confused by "faddism" (Hamer) [Lustick 1993, 169].[21]

It probably would have been possible to identify the particular political and methodological biases of these different authors and then, by discounting those sources which seemed least convincing and most biased, to accept as the "true" account the story told by the source(s) that seemed most reliable. But by doing so I would have exposed myself to the criticism that, for this crucial episode, the sources I would find most convincing, least biased, and most reliable would be those which fit most conveniently with the specific requirements of my own argument. I also would have been forgoing the opportunity to treat each of these works as a data point, using my theory to explain patterns of overlap, omission, and controversy across this data array and so test and, it is hoped, corroborate the argument. I therefore included in my treatment an analysis of patterns in the historiography itself.

First, I noted that most of this literature reflects a revisionist attitude toward older hagiographical treatments of Gladstone which pictured him as a farsighted, heroic, but ill-starred statesmen ready to sacrifice his career for that which he knew was right. Since these historians were, despite differences among them, seeking to revise this older view by citing the self-interested motives that were or could have been present, it would have been easy to cite all these studies as sufficient proof that Gladstone was acting in the kind of "entrepreneurial" way my theory expected. To an extent I did this, noting that, "despite their differences, these scholars clearly established how crucial were Gladstone's partisan political calculations in linking structural changes in British society and in the electoral system to a redefinition of the Irish question in British politics" (1993, 169).

I went on, however, to observe that my theory required Gladstone to have been operating in this entrepreneurial mode for several decades and noted that this is precisely what Gladstone had said he had been doing. That historians had failed to take note of the discursive shifts Gladstone made in the 1860s and 1870s on Ireland-related issues was due, I argued, to their parochial historiographical obsession with the "event" of Gladstone's "conversion" and with their general intent to "debunk" claims of Gladstone's generosity and powers of insight. I thus sought to justify the lines of inquiry and argument encouraged by my research program by showing that they led to important meta-historiographical insights. At the same time, the historiography itself was used to construct a necessarily incomplete but still theoretically nondeterminative narrative.

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[21] The phrase "eccentric historicism" comes from Dunne (1987). This quotation is from Lustick (1993, 169).

Thus, I drew four kinds of support from analysis of the historiography for my theoretical claim (about the role of political entrepreneurship in Gramscian wars of position): (1) by using material in it to construct a record of what happened based on the overlap in the accounts of various historians who differ methodologically and substantively; (2) from the "new fact" I predict about Gladstone's earlier entrepreneurial manipulation of the Irish issue (reflected in his contemporaneous diary notations and his retrospective interpretation of his behavior); (3) from the consistency of this "new fact" with the fundamental argument of the new historiography on Gladstone's conversion that emphasizes his posture as a partisan politician; and (4) from the extent to which the findings and arguments of generations of historiography on Gladstone's conversion can be mapped and explicated using the categories, propositions, and expectations of my theory. Taking these various uses of the historiography into account, it is less correct to understand my focus as on the one "case" of Gladstonian entrepreneurship and more correct to view my argument as based on the much larger number of treatments of his political style and strategy. It is with reference to this much larger "N," regarding this particular episode, typical of the scores of episodes that comprise the various background narratives of my study, that my theory is being tested (1993, 165–74).

Let us consider the problem and possible solutions in more general terms by listing four strategies for coping with or exploiting the multiplicity of partially inconsistent historical monographs.

## 1. Be True to Your School

Instead of treating all historiography as one unsynthesized but synthesizable Historical Record, and instead of picking and choosing from all historiography the bits one needs for a most convenient background narrative, one strategy available to researchers is to grant explicit consideration to the historiographical terrain at the outset of the study, identify the particular approach or school of historiography whose work is most convincing, and indicate its distinctive (even if implicit) theoretic commitments and biases. The researcher could then state that the history produced by this chosen school would, for the purposes of the study or for the research program within which the study is embedded, be treated as if it were (so far as is known) a description of what actually occurred in the past. Readers (as well as the author) would thereby be aware of the boundaries of the claims made in the study. To the extent possible, the researcher could choose a school whose implicit theoretical assumptions ran counter, or at least not parallel, to the theory entertained by the researcher.

One problem with this approach is that schools would often be too narrowly focused by region, period, or subject for researchers to produce the range of material necessary to construct the detailed background narratives required for testing sophisticated general claims. As the number of different approaches or schools from which evidence is drawn expands, the utility of this strategy for coping with selection bias diminishes. Nonetheless, by focusing attention on those aspects of a study which entail drawing evidence from sources outside the primary historiographical tradition being exploited, authors would help alert both themselves and their readers to claims for which other coping strategies, outlined below, should be applied.

## 2. Explaining Variance in Historiography

Another approach, based on the practice of historians as described by Mink and White and reflecting in part what I sought to do in my treatment of Gladstone, would be to presume that the narratives which comprise available historiography are likely to reflect, ceteris paribus, a normal distribution of implicit theoretical commitments. The richer and longer the historiographical tradition involved, the more confidence one could have in such an assumption. In any event, on the basis of this assumption, real analytic significance could then be attributed to regularities which appear in this historiography *despite* these (implicitly theoretic) differences. Corroboration for one's theoretical claims would then be sought in the success with which predictions could be made about points of concern, convergent claims, or divergence in historians' discussions of particular trends and episodes.[22] Just as historians, in effect, predict or explain patterns that others seeking to replicate their work will find in sources as yet unconsulted or artifacts as yet undiscovered, so would social scientists, using this approach to historical monographs and the historiographical discourses they produce, predict or explain patterns in the work of historians themselves, including works not consulted or even, perhaps, not yet written. The more powerful the theory, the more about historiographical patterns could be explained, *despite* the influence of sociology of knowledge factors on the orientations and conclusions of historians working in relevant and proximate fields.

Applying this technique means acknowledging that the background narrative against which the theory is to be measured is established by drawing on a range of historiography that, however differentiated, will still at some paradigmatic level of abstraction reflect the positive and negative heuristics, that is, the bets about what kinds of factors are relevant and irrelevant to the inquiry, which form the basis of any coherent research program. This technique also requires that presentation of the background narrative be placed in the foreground. Rather than present the background narrative as a prepackaged story line whose authority is justified only by reference to an undifferentiated collection of what are, in fact, disputed, heterogeneous, and partially conflicting historical works, the accounts of episodes containing the patterns to be explained must be justified by direct attributions to works by different historians, or different historiographical clusters. This means abandoning quite common practices, such as citing sources which give a convenient story without noting that other (partially conflicting stories) are available in other sources or authorizing convenient sources by labeling them as "excellent," "outstanding," or "influential" without noting or justifying the standards used in applying those adjectives. Shifting attention to the explanation of patterns in the distribution of that historiography means accepting a trade: a trade of reduced claims about verisimilitude in the relationship of our models to the reality of the past in return for a much larger number of cases (each work of history to be taken as a case) with which to test our hypotheses, models, and theories.

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[22] Of course, this technique must be discounted to the extent that historians, seeking to act as "social scientists," move in a clustered fashion to apply in their own work particular, fashionable theories produced by social scientists. For an extended discussion of how historians, consciously and unconsciously, import social scientific concepts and theories into the organizing framework of their narratives, see Burke (1992).

### 3. Quasi-Triangulation

Another technique, also reflected in my Gladstone illustration, is to construct a background narrative from the identity of claims made by different historians despite their approach from different archival sources and/or implicitly theoretic or political angles. In other contexts this has been called "triangulation," and it certainly increases the level of confidence we may have in the claims made about the past. There are two problems with relying solely on this approach. First, it is unlikely to produce enough overlap on enough topics to flesh out a full-scale background narrative for any but relatively simple and discrete comparative historical studies. Second, even when convergence is achieved, that fact does not deliver the precision implied by the trigonometric term. In trigonometry, triangulation requires complete knowledge about the theoretical and empirical relationship among the sources of the different lines of sight. Few researchers would be in a position to claim that they have a theory capable of organizing the different approaches of the historians whose work they are "triangulating" within the same conceptual/theoretic field.

### 4. Explicit Triage

Confronted with a variety of historical accounts inconsistent in ways directly pertinent to propositions under examination, scholars might choose to share the qualitative judgments that led to choices of particular sources for constructing different parts of the background narrative. This might well involve exposing the extent of stylization by explicitly describing different pieces of the background narrative, as the account of individual historians or groups of historians. This technique would be supplemented by discursive footnotes that alert the reader to alternative versions and briefly explain the reasoning that led to the rejection of these accounts. By demonstrating awareness of alternative views and providing the analytic justification of their choices, authors could build considerable confidence in their readers' minds that selection bias had been avoided—not least by providing them with the means to replicate the readings and inferences involved. One substantial drawback to this strategy, especially in light of constraints imposed by publishing houses, is that both the level of detail and the length of manuscripts could be significantly increased, a problem which might be dealt with by limiting use of this technique to elements of the background narrative that are either particularly controversial within existing historiography or that are particularly salient for the theoretical argument under review.

## CONCLUSION

By listing these four strategies I do not suggest that every aspect of an historically minded social scientist's stylized narrative be justified in one or more of these ways. Such a practice would prevent scholarship, not enhance it, even if focused on only one case or episode, to say nothing of the impossible demands it would make on anyone attempting to do a study comparing multiple cases. Rather, social scientists explicitly cognizant of the dangers of selection bias, of the absence of an unproblematic "Historical Record," and of both the limits and opportunities of diversity in the historiographical arrays available to them can choose where in their narratives they need to justify choices, how best to do so, and when to use patterns in the historiography itself as the target of analysis.

Whatever strategy or mix of strategies is followed, important benefits will accrue to both social scientists and historians. Each group can acknowledge the epistemological and methodological propriety of work carried on across a real, but usefully and necessarily bridgeable, disciplinary divide. Social scientists can use the "stories" of historians as overlapping but inconsistent "observations" of a past not directly accessible to anyone, or as surrogates for an unavailable master background narrative, thereby multiplying cases available for the testing of theories. Meanwhile, historians can read social scientific treatments of historical materials without the shock of sensing that the social scientists are selecting from available historiography to fit their theories or are using the work of historians (as a univocal source of the Truth about the past) in ways that contradict the methods and assumptions of the historians who produced it. Historians will also find, in social scientific treatments of historiography, new kinds of questions with which to interrogate the relics of the past and develop new story lines without feeling that to do so in a conceptually responsible way they must formally and systematically adopt any particular theoretical framework.

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# BOOK REVIEWS

## Political Theory

**The Elements of Social Theory.** By Barry Barnes. Princeton: Princeton University Press, 1996. 263p. $29.95.

**Politics, Sociology, and Social Theory: Encounters with Classical and Contemporary Social Thought.** By Anthony Giddens. Stanford University Press, 1995. 304p. $45.00 cloth, $16.95 paper.

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These two recent books, both by respected social theorists, offer a valuable look at the development of classical social theory since the middle of the nineteenth century and its relevance at the end of the twentieth century. In the final years of this century, it is highly pertinent to ask whether social theorizing largely produced before the end of the nineteenth century can possibly serve to illuminate contemporary society. (By classical social theory these authors mean roughly the theories of Marx, Durkheim, and Weber, along with subsequent elaborations and criticisms of these theories.) Are the "classics" of social theory of continuing relevance to our own attempts to understand and explain important social processes? Or have events and ideas superseded them? Giddens and Barnes have each done deep and important work on the traditions of social theory, so one reads these books with interest and high expectation. The books are highly complementary, in that Giddens provides a thoughtful historical discussion of the traditions of social theory, while Barnes offers a developed account of how the currents of this tradition ought to be deployed in contemporary social theorizing. Barnes wishes to provide a synthesis of an adequate social theory, taking account of the theoretical insights of classical social theory as well as well-developed criticisms of those theories.

There are several foundational questions to raise in the context of the topic of these books. What is the intellectual role of social theory? And how should the micro/macro problem be handled in good social research: What is the relationship between the individual level and the social level in social processes, structures, and functions? Barnes offers a possible answer to the former question in the initial sentence of his book: "The most widely read theories . . . are those that serve as the basis for commentary on the nature of present-day industrial societies and on how they are likely to develop and change" (p. 1). Later he writes, "the formulation of links and connections between particulars is what theorizing is" (p. 2). This is to say, evidently, that theory serves to identify the generalized processes that underlie specific social occurrences. Barnes takes explicit issue with what is sometimes referred to as the "toolbox" model of social theory: that social theory identifies a range of distinct social processes and mechanisms, and that the empirical researcher should be broadly acquainted with these theoretical alternatives and choose the theory that best fits the empirical problem that she is currently studying. Against this view, Barnes argues for theoretical unity (p. 33). It bears saying, however, that social phenomena may in fact be diverse, deriving from a number of nonreducible causal processes. In this case, one would be best advised to adopt the more eclectic or pluralistic view of the role of theory. If the social world is causally diverse, then there will be no "grand unification theory" possible in the social sciences.

A similarly foundational question focuses on the "social" in social theory: What is involved in asserting that human beings are social beings? Barnes is at pains to question a broadly individualist answer to this question, according to which social institutions constrain and impel rational individuals who are defined independently of social facts. He argues instead for the irreducibility of the social. (It might be noted that Barnes's critique does not apply merely to rational choice theorists; he believes that there is a pernicious individualism lying at the foundation of Parsonian sociology as well [pp. 54ff].) Against these individualistic social theories, Barnes stresses that we must understand individuals as thoroughly socialized: Their motives, beliefs, norms, inhibitions, and conceptions of themselves and their relations to others are thoroughly imprecated in social facts and circumstances. As Barnes puts it, "to insist that we are social creatures and to treat this as a matter of profound and pervasive significance is to announce a strongly anti-individualistic sociological theory" (p. 4). He gives substantial attention to a number of social concepts: norms, groups, movements, practices, knowledge, class, and hierarchy. And he aims to persuade readers that an acceptable social theory will be one that gives full recognition to the supraindividual properties that such concepts invoke.

Barnes appears to believe that familiar problems of suboptimality of rational individual choice in circumstances of interactive action—free-rider problems and prisoner's dilemma problems—indicate a defect in the theory of individual rationality. But it is surely no critique of evolutionary theory to demonstrate that suboptimal adaptations may emerge through natural selection; it is rather a feature of the world that evolutionary theory discerns and explains. Likewise, the fact that rational individuals placed in a one-time prisoner's dilemma situation will choose strategies that lead to a predictable suboptimal equilibrium does not refute the theory of economic rationality; it indicates only that individual rationality does not always lead to collective optimality.

Barnes believes that an appropriately constructed theory of the social individual permits a solution to the collective action problem (pp. 4, 76–85). Barnes is right in focusing on collective action as a theoretically central problem for the social sciences. But it is a problem for all stripes of social theory, not merely rational choice theory. It is true that unadorned rational choice theory predicts too little collective action. But a lavishly *social* theory of collective action surely predicts too much collective action. The assumption of social behavior (that is, behavior oriented toward group purposes) makes the problem of collective action too easy. If individuals always choose to act according to their assessment of collective interests, strikes should never collapse, revolutions should never fail, and groundwater sources should never be polluted.

Barnes gives substantial attention to the topic of social norms and what is involved in an individual following a norm. He writes that "conforming to norms is a collective activity" (p. 57). More broadly, Barnes maintains that the phenomenon of normative action gives strong support to the anti-individualist methodology that he favors. This position is unpersuasive, however, in that it appears relatively simple to provide "microfoundations" for the influencing of individuals by norms, practices, and worldviews that extend beyond their own psychological states. To put it crudely, the Parsonian model of the internalization of norms that Barnes criticizes appears to be more credible than Barnes's own account. In general, Barnes overestimates the difficulties of framing an empirically adequate individualist (or microfoundational) theory of norms and institutions.

In sum, Barnes's book repays close study. He is a thoughtful commentator on foundational questions in the social sciences, and even if one ultimately disagrees with his position, the reader will learn much from his discussion of these topics.

Turn now to the Giddens volume. This collection of previously published essays provides a fine discussion of and commentary on the main contributors to classical social theory—Marx, Weber, Durkheim, Comte, and Parsons—and a suggestive reflection on more recent social theorists—Garfinkel, Habermas, Foucault, and Nietzsche. Giddens has much of interest to say about the development of social theory and sociology over the past century and a half. He himself is a distinguished contributor to this tradition, so his discussion will be sure to interest many readers.

The heart of classical social theory is the trio of Marx, Durkheim, and Weber. Giddens takes pains to scrutinize the routes by which the thought of these thinkers made its way into the English-speaking world of social theory and sociology. Giddens's treatment of Weber is particularly thorough and useful; he makes a sustained effort to explore the connections between Weber's intellectual development and the social and political circumstances in which he found himself in Wilhelmine Germany, through an analysis of Weber's political writings. Giddens also provide a nuanced discussion of the relationship between Marx and Weber (chap. 2). Here the issue largely concerns the status of historical materialism as an explanatory framework. Giddens argues that Marx and Weber in fact shared a good deal of common ground concerning their interpretations of Roman history and precapitalist forms of economy and society (pp. 71–4). (Refreshingly, Giddens also defends the view that Marx remains a "classical social theorist," with important contributions to make to our understanding of the dynamics of modern capitalist society and economy, in spite of the collapse of communism.)

Durkheim's thought is reconsidered through the lens of his political sociology. Giddens takes the view that other readers have given too little attention to Durkheim's efforts to make sense of forms of authority in modern society. This concept cuts across typically sociological discussions of order with political discussions of power. Durkheim is most commonly associated with the regulative authority of norms and of organizations of civil society, but Giddens argues that there is equally a concern for the exercise of the coercive power of the state (pp. 102–7). Giddens offers the summary assessment: "Durkheim's sociology had its origins in an attempt to reinterpret the claims of political liberalism in the face of a twin challenge: from an anti-rationalist conservatism on the one hand, and from socialism on the other" (p. 115). Giddens also provides an extended discussion of Durkheim's relationship to individualism. Durkheim is plainly hostile to methodological individualism as a principle of sociological explanation; less well understood is his relation to "moral individualism." In Giddens's telling of the tale, moral individualism is, according to Durkheim, a defining characteristic of modern Western social life. "Respect for the individual, and the concomitant demand for equality, became moral imperatives: as such, they implied that the welfare and self-fulfillment of every member should be sought after" (pp. 121–2).

In a central essay in the volume, "Comte, Popper and Positivism," Giddens gives an historical treatment of the development of the philosophy of science in relation to sociology. The general framework of the chapter is a careful and developed analysis of positivism as a conception of the methodology of the social sciences. Tracing the concept from Comte through the Vienna school philosophers (Schlick, Neurath, Carnap, and Hahn, for example), to the logical empiricism of Hempel, Oppenheimer, and Nagel, Giddens carefully works through the relationships between philosophical debates about positivism and the development of empirical sociology. This discussion sheds much light on the profound impact that logical positivism had on the formation of sociological thought and method. This chapter by itself would serve as an admirable introduction to the philosophy of science for graduate students in the social sciences.

The final few chapters offer discussion of several important contributors to social theory in the twentieth century—Habermas, Marcuse, Garfinkel, and Foucault. These essays have the feel of occasion pieces rather than fully developed analyses, but each essay provides some insight into the subject at hand.

In sum, each of these books must be counted as an important contribution to contemporary discussions of social theory. Giddens's book shows a remarkable erudition in the full range of social theory. Giddens offers insight into the logic of the views he discusses, as well as the complicated relations that obtain among them. The book will appeal as strongly to readers who know a good deal about these traditions as it will to the novice. The Barnes book has a distinctive point of view and makes a substantive case for a particular approach to social theory. Some readers will find this approach highly congenial to their own work and other will not. But all will be stimulated by Barnes's capable argumentation and exposition of the issues. Barnes and Giddens each give evidence to what is surely true: that the traditions of classical social theory continue to provide insight into modern society.

**Nietzsche: The Ethics of an Immoralist.** By Peter Berkowitz. Cambridge, MA: Harvard University Press, 1995. 336p. $35.00.

Bruce Detwiler, *Florida International University*

Peter Berkowitz's *Nietzsche: The Ethics of an Immoralist* is a brilliantly provocative meditation on Nietzsche as an ethical theorist committed to an account of the best life. In response to the heavy emphasis on Nietzsche's philosophical radicalism in recent decades, Berkowitz emphasizes the extent to which Nietzsche actually embraces some of the traditional conceptions he purports to reject, especially with regard to truth, nature, and morality.

Berkowitz also criticizes the standard practice of making arguments about Nietzsche's thought on a given topic by "picking and choosing, . . . cutting and pasting words, phrases, and ideas drawn from wherever they can be found in Nietzsche's *Collected Works*" without reference to context (p. 10). Arguing that Nietzsche wanted his books to be read as unified wholes, he builds his discussion around an analysis of six texts considered one at a time.

He quickly dismisses the common view that issues of language and interpretation are the central ones, arguing instead that Nietzsche's fundamental concern is with ethical and, therefore, political questions. In Berkowitz's view Nietzsche's "antimoral propensity" is rooted in a "countermorality," an alternative conception of what is good, right, fitting for a human being (p. 48). Moreover, he argues that this countermorality is not an arbitrary creation devoid of grounding in truth. He rejects the view that Nietzschean perspectivism means there is no way to assess the validity of competing accounts of the world. "For Nietzsche," he explains, "the death of God—that is, the denial that nature, reason, or revelation provides moral standards for the governance of life . . .—is the one true account of the circumstances in which human beings really dwell" (p. 3). This "true account" and various metaphysical insights loosely associated with it are fundamental to Nietzsche's countermorality.

Berkowitz acknowledges Nietzsche's many assertions to the effect that the will to truth is the will to power and that all values are human creations. Nevertheless, he also finds in Nietzsche an "unyielding conviction that there is a knowable and binding rank order of desires, souls, and forms of life" (p. 262), suggesting that there is an unresolved tension between knowing and making, discovering and creating pervading Nietzsche's thought. This is at the heart of what Berkowitz characterizes as a "contest of extreme and conflicting views" (p. 4) characterizing all of Nietzsche's writings and lending to them an extraordinary unity. The contest between creating and knowing and between Nietzsche's ethics of creativity and his ethics of knowing is resolved to a degree (but only to a degree), according to Berkowitz, by the recurrent implication that "right making is based on right knowing." In the end, Berkowitz concludes that Nietzsche pushes the antagonism between knowing and making to the breaking point; the order of rank upon which so much Nietzsche's countermorality depends cannot survive the alleged discovery that the source of all order is the human will (p. 269). As a consequence, Berkowitz wonders whether what Nietzsche calls the death of God, if embraced unequivocally, might not prove to be as debilitating a myth in Nietzschean terms as those it seeks to displace.

This is an insightful and courageous book that raises important questions and considers fascinating possibilities, however controversial some of its interpretations. Berkowitz's masterly account (the best I have seen) of *The Uses and Disadvantages of History for Life* informs his subsequent discussions of *The Birth of Tragedy*, *The Genealogy of Morals*, and *The Antichrist*, where he argues that Nietzsche's primary objective in his historical writings was not to understand history or to deconstruct it but to mine history creatively for its lessons about what is of highest value.

Berkowitz's emphasis on Nietzsche's preoccupation with ethical questions is thoroughly appropriate, and his argument that Nietzschean perspectivism does not preclude Nietzschean knowledge is convincing (there may be no facts, only interpretations, but some interpretations are far more credible than others, and one can *know* this). Similarly, Berkowitz is persuasive when he argues that Nietzsche's praise of self-creation has limited parameters. Nietzsche has in mind a particular kind of self-creation that is compatible with intellectual integrity as he conceives it.

There is, however, an important respect in which Berkowitz may seriously underestimate the extent of Nietzsche's radicalism. He takes Nietzsche's repeated insistence on an order of rank among values to mean there are "objective and intelligible moral distinctions" in the world (p. 248), however categorically Nietzsche seems to repudiate that inference. According to this interpretation, the Nietzsche who believes the genuine philosopher to be a creator and legislator of values is at war with the Nietzsche who believes the highest values are part of the intelligible furniture of things. In my view, the textual evidence for the latter Nietzsche tends to dissolve once one takes him at his word that he himself is a philosopher, which presumably means a creator and legislator of values, including the values championed in his own writings. That those values are compatible with an intellectually honest interpretation of the world in no way makes them inevitable or objective. The alternative is to render much of what is most original in Nietzsche's thought not just paradoxical but fundamentally incoherent.

These criticisms aside, this is an important book. It is deliberately provocative, but the high quality of so many of its provocations makes it a must for those interested in Nietzschean ethics. Berkowitz conducts a highly intelligent war against familiar positions with arguments that are always thoughtful, often convincing, and addressed to important issues.

**The Politics of Historical Vision: Marx, Foucault, Habermas.** By Steven Best. New York: Guilford, 1995. 294p. $23.95.

Stephen K. White, *Virginia Polytechnic Institute and State University*

Today we often hear pronouncements of the end of history. Sometimes these come from conservatives, such as Francis Fukuyama, sometimes from postmoderns, such as Jean Baudrillard. However, Best would have us reject such conceptions. They represent nothing more than a failure of historical imagination, the effect of which is to foster political complacency. Today more than ever we need to cultivate our capacity to imagine "an alternative future that is grounded in knowledge of real historical dynamics" and inspired by "liberatory intent" (p. xv).

Guidelines for such a task emerge out of a confrontation of Marx, Foucault, and Habermas. Best compares and contrasts their insights, seeking plausible ways of reconceptualizing historical continuity, discontinuity, progress, rationality, difference, and freedom. In this three-cornered dispute, he shows admirable skill in drawing out the central concerns of each contestant and in carefully delineating their strengths and shortcomings. He is especially good at drawing a nuanced portrait of Marx, although some readers may find that the tensions in his work are stronger than Best admits.

This confrontation of theorists does not, however, produce much in the way of novel insight into their thought. Thus, for example, Foucault is applauded for correcting Habermas's failure to account adequately for how power informs discourse; but he is scolded for leaving us in a somewhat "apolitical" (p. 129) situation, at least with regard to liberatory projects. Marx is applauded for his attention to concrete political struggle, whereas Habermas is criticized for his abstractness and distance from the concerns of everyday politics.

The fact that most of Best's insights into these theorists are relatively familiar to contemporary political theory would not be a particularly serious problem for this book if his exegesis were merely setting the stage for the author's own distinctive departures. But the reader comes away somewhat disappointed on this score. It is not that he fails to raise interesting issues, but rather that he breaks off the analysis of them before his treatment has established much persuasive power.

A crucial issue in this regard is the conceptualization of the normative or liberatory dimension of a historical vision. Best takes Habermas to task for his ahistorical, universalist understanding of the emancipatory interest and of the normative presuppositions of speech. These shortcomings can be exposed, we are told, by the "postmodern pragmatism" (p. 226) of Foucault and Rorty, which tells us that normative criteria can only be "found immanent within a specific sociohistorical context" (p. 227). Best is, however, ultimately uncomfortable with this perspective, since it threatens to render his ideal of an emphatic, normative, historical vision somewhat problematic. Such an imagination is at risk when "local," historically varying standards hold unassailable sway. His solution is to recommend an "appeal to historically constituted norms that, *after rational consideration*, are recognized as a valid court of appeal" (pp. 228–9, my emphasis). What exactly does practical rationality mean here? Best does not really stop to untangle this knotty problem, except to assert blandly that valid norms must accord everyone freedom.

A similar problem emerges with the topic of metanarratives, that is, generalizing narratives that "discern and help realize the possibilities for human freedom as they develop throughout history" (p. 231). In a way, the momentum of the entire book draws us toward this issue. But Best's treatment of it, though sensible, is not pushed nearly far enough. The issue, of course, is whether it is wise to continue to construct metanarratives given the many trenchant critiques that have been launched against them. Best's answer is affirmative, provided we rid these stories of teleological, linear, and essentialist characteristics. Once they are so chastened, we can reemploy them to highlight crucial "*tendencies* in history toward progress, toward establishing the preconditions of freedom, toward a liberatory mediation of the universal and concrete" (p. 233). The result would be social and political theory animated by a "weak utopianism" (p. 254). I am actually in a good deal of agreement with such sentiments. What concerns me is that Best fails to develop his own notion of historical vision with sufficient persistence. It is an estimable goal to seek a generalizing narrative of freedom that is also "multiperspectival" (p. 133), but the reader simply needs to see such a conception fleshed out more in order to judge it critically.

A final problematic issue emerges at the end of the book. In a very brief epilogue, Best informs us that none of the theorists he surveys has made an "ecological turn" (p. 271), something that must become part of an adequate historical vision. Again, I have no difficulty with such an assertion. But the mere announcement of a problem that the author himself considers crucial, rather than a treatment of it, leaves the reader rather perplexed.

In sum, one wishes that Best would have provided less extensive surveys of Marx, Foucault, and Habermas and more sustained analysis of the problems one faces in trying to revivify our power of idealization in political life.

**Power in Struggle: Feminism, Sexuality, and the State.** By Davina Cooper. New York: New York University Press, 1995. 192p. $50.00 cloth, $17.95 paper.

Susan Bickford, *University of North Carolina at Chapel Hill*

Davina Cooper's fine book offers powerful analytic resources to a variety of scholars, including feminist theorists, students of grassroots activism and local government, and political scientists investigating the state (particularly the relationships between states and social movements). Cooper delineates precise and sophisticated conceptions of power and state identity, and demonstrates their significance through detailed analyses of particular political and social struggles around sexuality.

Cooper theorizes in very specific terms a multidimensional conception of power, one which is "analytically and normatively polysemic" (p. 7). She utilizes the best insights of Foucauldian analysis by understanding power as productive: what power does is generate effects. But Cooper does not shy away from the notion of individuals exercising power. Rather, she insists that our understanding of power must encompass "the intentional exercise of power" as well as "anonymously generated effects" and "the capacity (even if unutilized) to generate effects" (pp. 20–1). She distinguishes four interrelated forms of power: ideology (interpretive frameworks and discursive practices); force (physical or psychological coercion or threat); discipline (mechanisms that structure bodies, interactions, and institutions); and resources (tools or capacities that enable actions that have social impact). These modes of power operate concretely through diverse technologies (language, surveillance, legal rights) and sites (homes, schools, courts, bodies, states) (pp. 21–4).

With a sure hand and a perceptive eye, Cooper brings this complex account of power to bear on particular political sites and struggles. She begins with an analysis of various radical challenges to conventional understandings of sexual desire and sexual identity (chap. 3). She then draws on her analysis of the state (chap. 4) to discuss controversies over the access of single women and lesbians to state-run artificial insemination programs (chap. 5), and to provide an extraordinarily nuanced reading of lesbian and gay activists' engagement with municipal government in Britain (chap. 6).

The character of the state as an entity is a source of disputation in contemporary political and social thought. Is the state a coherent agent that pursues particular agendas, or is it fragmented, contradictory, indeterminate? Cooper reframes this issue by focusing on "state identity." State identities, she argues, are various and contested; states are "multifaceted phenomena with no fixed form, essence, or core." This lack of fixity does not imply utter openness. Rather, the notion of state identity "fuses material and ideological elements" in diverse and changing ways: The "sociophysical presence of state institutions" is bound up with discursive understandings and disagreements about the identities of the state and the principles on which it is legitimate for the state to act (pp. 60–1). (Some examples of state identity are the agent of legitimate coercion; the corporate body of the nation-state; a set of arenas or a particular account of linkages between public bodies; a democratic institution; a manifestation of "organizing principles" of race, gender, sexuality [pp. 62–3]). Different state identities are effects of actions and processes within and against the state, and one such effect can indeed be "the notion of a coherent state exercising agency to advance a specific agenda" (p. 94). These effects are real consequences with social impact; so whether or not we can say with ontological certainty that the state *is* a coherent actor, "the fact that it appears to possess agency requires us to examine the ways in which state structures and processes achieve this effect" (p. 85).

Since the state's essence is not fixed, neither are its boundaries or range. These aspects of state identity are irregular and contextually specific: sometimes they are permeable, sometimes solidified, often complex and contradictory. Cooper's approach to analyzing state power allows us to detect these ambiguities. For example, she argues that in the controversy over donor insemination, conservative politicians appealed to experts—doctors, social workers, psychologists—to validate their claims of the inappropriateness of fatherless families. Such expertise is constructed as knowledge from an objective, nonpartisan position; yet the employment of these experts in state-funded institutions indicates a more "ambivalent" location, "both within and outside the state" (p. 84). At the same time, since doctors at individual clinics could serve patients in ways that subverted conservative goals, legislation on this issue attempted to constrain and scrutinize the medical profession's autonomy.

Thus, neither the state nor social groups that work in and against the state are essentially unified actors. Cooper's fine-tuned attentiveness to power in all its manifestations results in a subtle, astute analysis of the disparate effects of lesbian and gay activism in local governments in Britain. The multiplicity of state identity means that actors can struggle against particular aspects of state identity while mobilizing others: "While such organizations tried to maintain a distance or separation from certain aspects of state identity—representative of elite interests, legitimizer of the status quo, reproducer of social/sexual inequality—they linked themselves to others—democratic, public funder, policymaking power" (p. 66). The success of such struggles is affected by the contextual limits (historical and discursive) to the state identities that can be coherently instantiated (pp. 112–20). (I will not attempt to describe her analysis of this case further; suffice it to say that her ability to construct a narrative that is theoretically insightful and empirically rich is noteworthy, and one is tempted to recommend her to students doing any kind of qualitative work, whether in cultural studies or comparative politics.)

There are two crucial corollaries to Cooper's understanding of power. First, just as there are multiple modes of power, there are myriad forms that struggle against power takes. Cooper argues against flattening out responses to power to mere resistance (the obstruction of processes of power). Responses can also include deploying power through the utilization of particular technologies in specific institutional contexts; changing or constraining certain forms of power (e.g., military might); reallocating resources or access to resources through "anti-poverty initiatives, community education, and rights-based struggles"; and exposing the workings of power, making it visible as power rather than as nature or necessity (pp. 130–1). Cooper does argue for the utility of intervening in the processes of power rather than simply reacting to effects (p. 138), but she also contends that radical social change requires "strategic pluralism," a variety of different practices and approaches at a variety of sites. We need (Cooper argues persuasively) to make contextual judgments about the possible effects of strategic actions and to extend generosity toward those who pursue strategies that are not our own.

Both generosity and judgment; this is not a counsel of relativism or eclecticism. Thus, the second corollary to Cooper's notion of power. Against the insistent despondency of some contemporary thinking, Cooper contends simply that if power produces effects, then it can produce oppressive or empowering ones, inequality or equality. To be able to say that power is not automatically domination implies the ability to distinguish between better and worse effects of power; it implies, in other words, a normative framework for analyzing power effects. Thus, Cooper joins a distinguished company of theorists (Chantal Mouffe, John McGowan, Iris Young) attempting to articulate a politics—and a political theory—that is at once normative and keenly aware of the problems of normativity. Although it is implicit throughout the book, it is only in the afterword that Cooper presents a direct argument for a particular normative gauge. To address the insufficiencies of contemporary accounts that stress freedom, equality, or harm, she offers an ethic that focuses on power itself. The normative judgment undergirding her analysis is that "people should have an equal capacity to make an impact . . . an equal capacity to achieve their notion of the good life and to contribute to the ways in which civil and political society are shaped" (p. 147). This does not mean that all preferences and all desires are equally valid. In arguing for an ethics of equal empowerment, Cooper does not reduce the notion of empowerment to subjectively felt affirmation. We can and should argue against the enactment of desires that reproduce or reinforce asymmetries of power, since they hamper "sustainable equality of empowerment" (pp. 148–53).

A guide to normative judgments that is grounded in power is appealing, but there is much more to investigate about its problems and possibilities (Cooper herself notes that her account is preliminary and tentative). For example, it is unclear to me that her response to lesbian sadomasochism (pp. 148–52) is going to change any minds. But then again, she offers this ethics as a way to redescribe the terms of debate, not to provide specific answers (p. 153). Political actors will still disagree about what desires should be contested or constrained, as well as about the utility of various practices and the impact of different strategies.

How do we adjudicate disagreements about what counts as "disempowering desires," and how do we make judgments about strategies in a context of uncertainty? With respect to strategies, as noted, Cooper stresses generosity and diversity but also contextual analysis and strategic learning (pp. 131–5). Under what conditions might we argue that particular strategies are dangerous and to be avoided? Cooper raises this question, but does not answer it here. It would certainly be unfair to expect her to provide a comprehensive account of political judgment when so many contemporary theorists struggle with precisely that. But in further developing "an ethical principle of sustainable equality of empowerment" (p. 5)—a principle that in no way guarantees consensus—Cooper will need to consider what counts as a principled practice of disagreement and debate.

Cooper's book is well worth reading, both for her substantive insights and as a model of how to do political analysis. Cooper clearly and without fanfare negotiates between poststructuralist and modernist (liberal or radical) analyses of power and subjectivity. She has an eminently discerning sense of what analytic resources are necessary for examining political and cultural phenomena, and this enables her to articulate a theoretical perspective that illuminates agency and discipline, empowerment and oppression. Her work promises to be as useful for developing theory as it is for thinking about strategies for change.

**Plato's World: Man's Place in the Cosmos.** Joseph Cropsey. Chicago: The University of Chicago Press, 1995. 227p. $29.95.

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In *Plato's World*, Professor Joseph Cropsey presents interpretations of eight Platonic dialogues. Seven of them are set by Plato at or toward the end of Socrates' life and thus "by Plato's explicit indication cohere as a Socratic valedictory" (p. x). As for the eighth, Cropsey drops the *Cratylus*, which may well belong to this group of dialogues, and reasonably adds an interpretation of the *Protagoras*, which has important links with a number of the other dialogues that he treats. Each of his eight chapters is devoted to a single dialogue, which he takes up in the order in which the conversation or episode it presents is supposed to have occurred in Socrates' life; yet those chapters are intended to form parts of a unified work, just as the dialogues they treat themselves cohere in some way. This is a work likely to attract a wide and attentive audience, drawn to it not only by the distinguished reputation of its author but also by the appeal which its portrayal of Socrates as "caring philosopher" will have for many people. The reviewer's task is surely not to praise it, therefore, nor even to call attention to its finer passages, but rather to attempt to indicate what can be learned for the study of Plato from the author's approach as a whole.

The first point to be considered then is Cropsey's respect for Plato, whose study he takes up in order to learn *from* him and not, or not merely, *about* him. From this stance, apparently so elementary, flow large and important consequences: openness to the possibility that Plato was fully aware of any flaws in the arguments of his spokesmen that a scholar of today might notice (e.g., p. 202), attentiveness to the rhetorical functions that some of those arguments may be meant to serve (see especially the chapters on the *Crito* and *Phaedo*), heightened concern with the question of the deeper coherence of arguments and themes within each dialogue and from one dialogue to another (e.g., p. 22). Cropsey does not defend his respectful stance or explain the reasons that led him to it, content apparently to let the results of his study speak for themselves in this respect. As for the reason which one can infer from his occasional references to more recent thinkers such as Machiavelli (p. 35) and Nietzsche (p. 176) as well as to doctrines regarded as modern (e.g., pp. 29, 119, 186)—namely, his view that Plato anticipated the most important insights of modern philosophy, while avoiding its mistakes (e.g., pp. 144, 186)—that reason presupposes on the part of the reader an acceptance of the legitimacy of the philosophic enterprise to begin with.

The second general feature of Cropsey's approach that must be considered has already been touched on: his willingness to take his bearings by the "dramatic order" of the dialogues, that is by the order in which, according to "Plato's explicit indication" the events presented in them are supposed to have occurred. In proceeding in this way, Cropsey distinguishes himself from the many scholars who attempt to take their bearings by the putative order of the dialogues' composition (p. ix). In justification of his procedure, Cropsey notes that, while the dramatic order of at least some of the dialogues can be known "beyond a doubt," the project to establish the order of their composition is likely to "continue to generate uncertain results" (p. ix). Indeed, one might have gone much farther here than Cropsey—who grants that "it would be important to know the order in which Plato wrote his works, so that one might . . . be instructed by the progress of Plato's understanding" (p. ix)—was apparently willing to go. Plato gives not a single indication that his understanding underwent a "progress" during the period of his composition of the dialogues. Those same dialogues, however, call unambiguous attention to a most important change, a progress, in Socrates' thought: the so-called Socratic turn. Cropsey treats that turn when the dialogue he is discussing compels him to do so (pp. 202ff., 206ff.); but, as it appears to me at least, he does not place much weight on it. He surely regards the "safe" answers (to the question of cause) that are its first fruits as "stupid" or "simple," adding "nothing to the understanding" (pp. 220, 208–10). And it may be for this reason that he is able to conceive of Plato's having "achieved" in the dialogues treated in the book what Cropsey calls "his cosmography" (p. 2). Hence the full title of the book, as well as its unifying theme. So far as I can tell, this "cosmography" has two principal teachings (whose possible link, in Cropsey's view, is suggested on pp. 104–5). According to the first, the "whole" has an "impenetrable resistance . . . to being known in its entirety" (p. 34); indeed, even the "boundary" between "the accessible and the incommensurable in the whole . . . must itself necessarily belong among the mysteries" (p. 59). The second concerns "man's solitude in the whole" (pp. 42, 121, 141–3 among other places). This means, in particular, our abandonment by the (nonexistent) gods, an abandonment which is presented not merely as a matter of "conviction" (p. 42) but as something which "we know" (p. 89; compare p. 122). But, if the whole remains always to some extent mysterious, how can we know that we are abandoned in it?

The emphasis that Cropsey places on cosmology or ontology or metaphysics is the other side of a relative disregard not so much of morality as of the moral investigations of Socrates and of at least one aspect of his interrogations of his fellow-citizens and others on these matters. Cropsey suggests that whatever Socrates may have *learned* from those interrogations could hardly by itself have justified his long preoccupation with them (p. 156). Despite the fact, then, that Cropsey is also somewhat impatient with the interrogations insofar as they posed a threat to the beliefs that nourish civic life (p. 224; compare p. 132 as well as p. 25), he ascribes them ultimately to the "caring" of the philosopher, who, as philosopher, "understands more clearly than anyone else the human need for human caring" (pp. 147, 157, 161). Caring is, as it seems to Cropsey, the appropriate response to "the human loneliness in the cosmos, which pervades our being" (p. 121) and thus, even more than the courage required to confront or accept the necessary limits to our knowledge (pp. 26, 70, 150; compare p. 176), the distinguishing mark of the true philosopher (p. 110 and p. 147).

**Reconstructing Political Pluralism.** By Avigail I. Eisenberg. Albany: State University of New York Press, 1995. 211p. $16.95.

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Eisenberg argues that "a reconstructed theory of political pluralism is primarily committed to offering individuals the means to a healthy personal development" (p. 171). This central commitment is not satisfactorily addressed, she maintains, by either contemporary liberals or communitarians. The liberal-communitarian debate, therefore, is problematic not only because the contending parties do not adequately confront each another's arguments but also because each side fails to account for the raw material needed to make its argument work. Neither addresses, adequately, the issue of "healthy personal development." Without a careful focus on the "healthy individual development" (p. 53) of citizens, both liberals and communitarians risk undermining their projects before they begin. Unless individuals are strong enough to judge wisely among the choices presented to them, their individual rights, however central to the pluralist political process Eisenberg develops, will not be sufficient. "A plurality of contexts is . . . required," she asserts, "to empower the individual and to cultivate in her good judgment through the development of critical perspectives" (p. 185). Similarly, though community support may be central to such empowerment, "communities of choice," cultivated by political pluralism, are far more likely to promote healthy individual development than are the "old and possibly oppressive attachments" (p. 186) of what she labels "nonvoluntary" and "involuntary" communities.

Many of these insights are not new, but they have been lost, Eisenberg suggests, due to the tendency of American political science to identify the entire pluralist enterprise with post–World War II interest group pluralism. The pluralist tradition is a far older and broader one—rooted in the writing of Dewey and James in the United States and in that of Figgis, Cole, Laski and Mary Follett. The work of these earlier pluralists is particularly compelling in that they "deal critically with the fact and the effects of domination" (p. 54) in a way that neither communitarians nor post–war, American, interest group pluralists do. Eisenberg insists that the contribution of these early pluralists must be revived and linked to the general enterprise of political pluralism that she commends. Moving beyond concerns with stability and socialization that Eisenberg identifies with the post–war pluralists, earlier pluralists, especially Follett, allow for the "establishing of a link between the political power of groups and the self-development of individuals" (p. 85), which, she claims, post-war pluralists fail to appreciate and which are lost by many liberal and communitarian commentators in recent years.

Examining liberal positions (for whom Rawls is the major representative) and communitarian thought (identified here primarily with the work of McIntyre, Taylor, and Sandel), Eisenberg urges political pluralism as an alternative that mediates between the two. While critical of both liberals and communitarians for falling short of the "normative commitment to ethical individualism" (p. 170) required to avoid what Dahl ("Pluralism Revisited") calls a "stable equilibrium of inequalities," her harshest words are directed at contemporary communitarians. The latter reduce "self-development" to "mere socialization," she argues, ironically repeating the central shortcoming of post-war interest group pluralists. Communitarians' concern for the "social context" of healthy self-development stops short, she insists, of an understanding of healthy self-development itself.

Unfortunately, Eisenberg's account also lacks a clear definition of *healthy self-development*, and so a major tenet of her position is left unspecified. Her plea for differentiating ethical individualism from what she labels "methodological individualism" and "atomistic individualism" is founded on her insistence that "without ethical individualism, personal development is reduced to mere socialization" (p. 126). It is not, however, clear of what this "development" consists. A multiplicity of groups offers, she suggests, choices, apparently liberating individuals from the oppression of both "nonvoluntary" and "involuntary" groups. The basis upon which these choices are to be made is, however, unclear, save that *more* choices are presumably better. One wonders, however, if the very maintenance of multiple associations requires a personal strength and focus, the source of which remains unclear in Eisenberg's case for pluralism. As Michael Walzer notes (*Thick and Thin*, 1994), "Pluralism makes for freedom because it leaves room for the canniness of the individual. But there does have to be a self capable of canniness, strong enough to absorb all the criticisms directed against it and to judge among them." Eisenberg would do well to take Walzer's comment seriously. Moreover, an old problem recurs: Multiple memberships (even in voluntary associations that she endorses as developmental) may weaken the impact of any single group, rendering membership in it less significant for the individual as the group's power diminishes. This is, of course, what Madison prescribed in *Federalist* #10 and what interest group pluralists admired. For Eisenberg, however, groups, especially a significant plurality of groups, are meant to empower, not diminish, the will of individuals.

In seeking to redirect our attention to the centrality of associational life of democratic citizens, this book resembles several recent works, notably those of Robert Putnam (*Making Democracy Work* (1993) and "Bowling Alone"). In recalling a pluralist tradition that values civic associations and the contribution they make to healthy democratic citizens, Eisenberg's work is important. In failing to confront and define the "self" that is to be strengthened and made healthy she leaves a fundamental question unanswered and casts doubt on her own pluralist framework. Democratic citizenship is nurtured, as Alexis de Tocqueville knew (*Democracy in America*, final chapter), only when individual citizens are made strong. That, he said, should be "the chief aim of any legislator in the age opening before us." With Tocqueville, Eisenberg recognizes the importance of developing citizens; she does not, however, define the scope of that development nor does she recognize the role that democratic political leaders may have to play in it. Tocqueville stressed the need for developing standards of judgment among a democratic citizenry even as he described the associational, pluralist processes of American public *and* private life. Tocqueville's insights, strangely absent from this book, are crucial to any commentary on associational life in the United States. He understood as a contemporary Tocquevillian, W. C. McWilliams, does (*How Democratic Is the Constitution*? 1980) that the aim of democracy is "self-rule," which "requires . . . that I be free to do what is according to nature. No barrier in my environment or in me must stand in the way. To help me toward self-rule, democracy must provide me with an environment that has resources enough to permit me to live in a fully democratic way." Erik Erikson put it in a slightly different way (*Identity and the Life Cycle*, 1959) when he asserted that "if we want to make the world safe for democracy, we must first make democracy safe for the healthy child." Presumably Professor Eisenberg would agree, but Erikson had a very clear sense of what "healthy individual development" was. Eisenberg's work requires that she develop a comparable clarity and understanding if it is to approach its admirable and ambitious goal.

**The Voice of the People: Public Opinion and Democracy.** By James S. Fishkin. New Haven, CT: Yale University Press, 1995. 204p. $20.00.

Robert Y. Shapiro, *Columbia University*

With his first American "deliberative poll"—the National Issues Convention (NIC), part of which was shown on public television in January 1996—James Fishkin has greatly enlivened debate about public opinion and democracy. Similar to the experiment he conducted in Britain in April 1994, which is described in *The Voice of the People*, a carefully drawn representative sample of 459 people participated, all expenses paid, in the NIC in Austin, Texas. From January 18 to 21 they were divided into groups; informed about national issues; talked about these issues in their groups; questioned experts, Republican presidential candidates, and Vice President Al Gore; deliberated some more; and were then polled to see if the opinions they reported in a predeliberation survey had changed.

While its jacket proclaims this to be "the book behind the PBS National Issues Convention," most of the book is not about the NIC itself. Rather, it offers an engaging view of the virtues of direct democracy that provides important standards for interpreting the NIC experiment—a better guide than Fishkin provides at the end of the book and later on TV. It emphasizes that the deliberative poll process reveals what individuals' policy preferences *would* look like (the better phrase is "*can* look like," since this is an experimental, not a real-world setting) if citizens were well informed, were exposed to more than the normal media sound bites, confusion, and efforts at political manipulation, and had the chance to talk to each other about the issues. Fishkin also attributes to the final poll responses a strong "recommending" force that policymakers, politicians, and the viewing public should heed since they resulted from an *informed* and *deliberative* process. As Fishkin's critics and many of his supporters understood, the NIC would be just a single—though potentially fascinating (which it was)—experiment structured around a certain range of issues (the family, the economy, foreign policy), experts, politicians, and a sample of citizens who knew for many weeks that they would be closely watched and shown on national television. Being closely watched had the potential for producing a Hawthorne—experimentation—effect.

The book's theme is that political life in mass democracies needs more conditions under which citizens can learn about issues, talk to each other about them, and have a way of conveying to the political process these reconsidered judgments after such deliberation has occurred. The book raises a major question: Is there a way to make governing more democratic in the spirit, if not the manner, of direct democracy in ancient Greece or American town meetings? Of course, working against such a development is the sheer size of a polity. As Fishkin crisply explains, the efforts that succeeded in giving the electorate more influence (direct election of more officials) and making the electorate widely inclusive (suffrage for women and blacks) ironically made direct democracy harder; led to less deliberation in the nation; and decreased, in rational choice fashion, the incentives to vote, to participate in other political activities (collective action problem), and even to become informed about political issues (rational ignorance).

In a way with which many (including myself) will disagree, Fishkin is critical of the normal qualities of public opinion and of any potential for the current system of political communications through the mass media to turn things around (though he duly credits the "public journalism" movement). He does not, however, do justice here to the extensive alternative arguments and evidence, none of which would dispute the desirability of more opportunities for deliberation. He readily concurs (or would concur) that the often dysfunctional mass media and low levels of direct participation have contributed to the public's current cynicism about politics, lack of trust in government, feelings of government's unresponsiveness, and the overall low level of "social capital" that Robert Putnam has recently written extensively about.

To remedy this public malaise, Fishkin resurrects the possibilities for direct democratic input through bodies of citizens chosen by lottery or random selection. This idea is clearly worth revisiting in light of contemporary politics, and there are clearly ways—some already tried or under way—to encourage and establish juries and large but manageable forums and groups of this sort. Fishkin emphasizes that participation in such democratic bodies contributes to the achievement of four important conditions or values: *political equality*, an equal counting of citizen preferences that can be viewed as represenfive; *deliberation*, the opportunity to consider competing arguments in face-to-face discussion; *participation*, direct engagement in the political process by a significant number of people; and *nontyranny* (of the majority), a process that avoids whenever possible the deprivation of any portion of citizens' rights or interests.

These are most worthy liberal democratic ideals. For broad thinkers about public education and civic engagement, expanding deliberative forums and opportunities (with our without accompanying opinion polls) is far more preferable and appealing than only a system of public debate and communication through the media to a passive public audience. Through active discussion and the additional information that comes with it, individuals should learn more about public problems and issues and have, as Fishkin emphasizes, more considered judgments, not top-of-the-head opinions that are normally picked up in public opinion surveys. This type of participation should also increase feelings of political empowerment. While this schema is consistent with enhancing political equality, deliberation, and participation, nontyranny is more problematic, however.

Nontyranny requires an appreciation of the needs and rights of others, which might be effectively increased by deliberative processes involving ordinary people representing the nation's *diversity*—that is, with people meeting and talking with others different from themselves, with whom they do not usually interact but whose concerns and claimed rights they only learn about through the media or talking with their limited circle of family and friends. Thus, the most useful forums would surely be those, like the NIC, that bring together randomly selected people (intentionally not allowing for self-selection or some purposive criteria as occurs in jury selection in the courts) to facilitate such out-of-the-ordinary interactions. As Fishkin playfully recalls about the first experiment in Britain (the same occurred at the NIC), survey researchers for the first time got to *see* one of their samples (so who was "*the* median voter"?). But more important, some members of the public at the NIC got to interact in a deliberative political context with a genuinely broad cross-section of America.

### Adam Smith's System of Liberty, Wealth, and Virtue: The Moral and Political Foundations of *The Wealth of Nations*. By Athol Fitzgibbons. New York: Oxford University Press, 1995. 214p. $42.00.

Roger J. Fechner, *Adrian College*

Athol Fitzgibbons's excellent work is a most welcome addition to the "new" Smith canon that has emerged over the past few years. His study is addressed to two very different groups of readers—scholars and the general public. This is a difficult task for any author who is dealing with a thinker such as Smith whose ideas are so complex and whose philosophical system is so wide ranging in subject matter, but Fitzgibbons has succeeded admirably. His thesis is clearly and succinctly stated in the opening lines of the preface:

> This book describes how Adam Smith set out to replace the Aristotelian philosophy of Western Europe, which had become a hinderance to liberty and economic growth, with an equally comprehensive but more vital world view. It sketches the great intellectual systems that Smith opposed, explains the significance of his Stoic religion, and shows why his moral philosophy led to a new economic blueprint. It also shows how, in the course of formulating his view, Smith was misled by the scientific credulity of his age. (p. v)

Fitzgibbon's study focuses on three central lines of argument that are major contributions to contemporary Smith scholarship. The first is the overall organization and structure of his analysis in which he demonstrates, through a careful and comprehensive reading of Smith's works, the fundamental coherence of Smith's system. Regarding the classic "Adam Smith Problem," Fitzgibbons offers fresh insights into Smith as neither an idealist nor a materialist. Rather, he characterizes Smith as a neo-Ciceronian stoic and social Newtonian who, above all else, wanted virtue to be revelant to this life and not the hereafter of the Aristotelian-Christian synthesis of antiquity. Fitzgibbon's reading of Smith's writings leads him to conclude that there is persuasive proof that Smith created a system of morals, politics, and economics that was both inclusive and interrelated. Smith's intention was "to *integrate* economics and morals, by developing a philosophy that would harness the force of self-love without being dominated by it" (p. 4). After proving the existence of and delineating the character of Smith's system in his introduction, Fitzgibbons analyzes Smith's scientific methodology and moral philosophy in Part II, his jurisprudential, political and social philosophy in Part III, and his economic thought in Part IV. Each section is neatly integrated with the others, and the whole progresses logically from moral theory to political theory to economic theory. Each argument the author makes is documented extensively with direct quotations from Smith's works. Fitzgibbons's entire discussion of Smith's system is marked by "unity and completeness" (p. 4), qualities that Smith himself treasured.

The second key argument of Fitzgibbon's study is his running debate with scholars, past and present, over their interpretations of Smith's ideas on a wide variety of subjects. Both in the text and in the explanatory footnotes, the author is absolutely clear and direct about his agreements and disagreements with the views of other Smith experts. Hence, Fitzgibbons not only offers his readers a fine analysis of Smith's system, but he also affords them a scintillating discussion of the historiography of Smith scholarship over the years. Since the author is an economist himself, one of the most striking examples of his debate with other experts is his evaluation of Fry's (1992) *Adam Smith's Legacy: His Place in the Development of Modern Economics*, a collection of essays by ten Noble Prize winners. Fitzgibbon's critique of the laureates' knowledge of Smith's thought couldn't have been more on the mark: "The most remarkable thing about [Fry's] book is that its contributors are so unfamiliar with many of the philosophic ideas that concern their own profession.... To judge by the evidence, not one of the contributors ... is qualified to pronounce on it [Smith's system]" (p. 171).

The third and most provocative argument of Fitzgibbons's analysis is his revisionist position that questions the standard scholarly conclusion that Hume's philosophical scepticism, humanism, advocacy of trade and the "benefits of self-love," and role as "a harbinger of positivist science" (p. v) had a determinative role in shaping Smith's thought. He successfully makes numerous subtle but telling distinctions between Hume's and Smith's philosophical positions on virtue, liberty, and wealth. Fitzgibbons argues that, unlike Hume, Smith's overriding goal was to give "liberalism ... a workable moral foundation, and that was his theme even in *The Wealth of Nations*" (p. v). Although the work is peppered with comparisons and contrasts between Hume's and Smith's thought, two examples will suffice to give a flavor of the author's discussion of their philosophical relationship. On the widely held argument that Hume influenced Smith to become an "amoral" thinker who supported "a materialistic and commercial society," Fitzgibbons concludes that "nowhere did he [Smith] say that his moral theory was meant to make society more materialistic, or that he wanted economic efficiency to replace morals" (p. 9). While Hume's major goal was "to introduce a new scientific method" into philosophical reasoning, Smith's was "to offer a new moral insight and demonstrate that a liberal society need not be undermined by its own lack of values" (p. 193).

The only reservation I have about this outstanding study is that Fitzgibbons has not used many of the excellent works published in the past few years, especially those that place Smith in his biographical context, such as Ross's (1995) *The Life of Adam Smith*, or in his intellectual context, such as Cairn's essay on Smith's jurisprudence and Dwyer's essay on Smith as an agrarian civic humanist in Mizuta and Sugiyama's (1993) *Adam Smith: International Perspectives*. Consideration of these and other recent studies would have made the author's analysis even fuller and richer.

Fitzgibbons's well-written work will appeal to a wide variety of scholars as well as the general reader. It is now the best synthetic analysis of Smith's system available and deserves a wide audience.

**Catholicism, Liberalism, and Communitarianism: The Catholic Intellectual Tradition and the Moral Foundations of Democracy.** Edited by Kenneth L. Grasso, Gerard V. Bradley, and Robert P. Hunt. Lanham, MD: Rowman and Littlefield, 1995. 271p. $64.50 cloth, $26.95 paper.

Simone Chambers, *University of Colorado at Boulder*

In the forward to *Catholicism, Liberalism, and Communitarianism*, a collection of 14 essays by political theorists, theologians, and legal scholars, Richard John Neuhause writes that the volume aims to convince the reader that Catholic social teaching (CST) "can contribute significantly to revivifying the American experiment in liberal democracy" (p. ix). The first step in this enterprise is to belie the suspicions of secular liberals who harbor a deep distrust (and misunderstanding) of religious arguments in general and Catholicism in particular. For those readers in doubt about Catholicism's commitment to liberal democracy, Kenneth L. Grasso—both in the introduction as well as in a lengthier contribution—offers a detailed and enlightening exposition of Vatican II's endorsement of constitutional democracy, human rights, and religious freedom. Also worth mentioning are Gerard V. Bradley's and Robert P. George's essays on natural law, which do a fine job of dispelling some of the most common misconceptions about Catholic social teaching—for example, that because it claims to be based on moral truth it must be antithetical to pluralism, civil liberty, toleration, and diversity.

Although concerned to show that CST is not illiberal, the authors are also very interested in maintaining a critical distance between themselves and the anthropological, metaphysical, and epistemological arguments of philosophical liberalism. Liberalism is blamed for leading us into a deep moral, political, and social crisis brought on by "corrosive skepticism," "moral relativism," "utter nihilism," and of course "rampant individualism." The focal point of this attack is a "notion of liberty as the freedom of an unencumbered, autonomous, imperial self to autoconstruct his or her own universe of meaning, value, and action, so long as no one gets hurt" (p. 257; see also pp. 6, 10, 19, 45, 93, 100, 173, and 226 for similar attacks on the liberal individual). There is nothing much new here and, as with a great deal of communitarian literature, the autonomous individual as the scourge of modernity and the cause of moral disintegration is somewhat overdrawn.

Although CST contains a number of interesting doctrines, the most important one is subsidiarity, which serves as a unifying theme throughout the book. Pope Pius XI described subsidiarity this way: "Just as it is gravely wrong to take from individuals what they can accomplish by their own initiative and industry and give it to the community, so also it is an injustice and at the same time a grave evil and disturbance of right order to assign to a greater and higher association what a lesser and subordinate association can do" (p. 87). Subsidiarity is grounded in the metaphysical doctrines of human dignity and perfectibility and is used to justify limited government, democracy, a sphere of personal autonomy, political and associational participation, and pluralism. "Subsidiarity stipulates that society should not be a unitary entity whose various agencies and associations are basically instrumentalities of the state; rather, the plurality of subsidiary agencies, associations, and smaller communities within the larger society must be understood as having a large measure of autonomy" (p. 251). Subsidiarity is Tocqueville with strong metaphysical backing.

The first three essays after the introduction—by Francis Canavan, Grasso, and Kenneth R. Craycraft, Jr.—offer the most detailed philosophical and theological account and defense of subsidiarity. Moving into political theory proper, Christopher Wolfe argues that subsidiarity offers "a better account of how limited government is rooted not merely in the defects of people (especially government officials) and their disagreements (the fact of pluralism), but in the positive goals they ought to strive for in community life as well" (p. 93). Jean Bethke Elshtain gives subsidiarity an ecumenical (indeed secular) endorsement when she admits to a hunch that "most Americans share the 'principle of subsidiarity' without calling it that or positing it in any formal sense" (p. 106). George uses subsidiarity to explain why the idea of world government entailed by natural law is not antithetical to pluralism: "For the natural law tradition, the obligation of international authority to respect cultural autonomy and diversity is rooted in subsidiarity and in the diversity of basic human goods and the upright and reasonable ways of instantiating these goods in the lives of individuals and their communities" (p. 146). The contributions of Michelle Watkins and Ralph McInerny, Mary Keys, and Robert Hunt, while concerned with the interpretive controversies surrounding the work of Jacques Maritain and John Courtney Murray (the architects of the Catholic rights revolution), deepen the reader's understanding of the rich philosophical resources and debates surrounding the concept of subsidiarity. As a Jew, Matthew Berke endorses subsidiarity because it limits the unhealthy influence of large secular agencies while at the same time encouraging the pursuit of human excellence and moral truth. And finally, George Weigel argues in the conclusion that subsidiarity offers an entirely new foundation to liberty and autonomy, one that counters the disastrous disenchantment brought about by the liberal conception of freedom.

Taken together, these essays make a strong case against those who would prefer to bracket all religious considerations from the public forum. Not only does such a bracketing amount to a "disenfranchisement of believers" (p. 86), but also it is based on the erroneous idea that all religiously inspired argument adds up to or can be reduced to "God says!" Religiously inspired argument is *argument*, which means that—just like any other argument we wish to introduce to justify our political ideals—it involves convincing with reasons. *Catholicism, Liberalism, and Communitarianism* sets out to convince Catholics and non-Catholics using reasons. The three non-Catholic contributors to the volume (Elshtain, Stanley Hauerwas, and Berke) are testimony that non-Catholics can find CST persuasive, and several other essays (I am thinking particularly of the contributions by Grasso, Bradley, George, Hunt, and Weigel) appeal to reasons whose genesis might be tied to Catholic teaching but whose persuasiveness stands on its own. The intelligence of the essays in *Catholicism, Liberalism, and Communitarianism* plus the generosity and humanitarianism of its political vision entitle this book to a serious hearing within the broader debate in political theory.

A small minority of the essays, however, offer a slightly different message, which might go something like this: "Catholic political theorists beware! In your eagerness to influence and participate in the public life of pluralistic democracies do not compromise your Catholicism." Hauerwas (a Protestant) carries this message most forcefully, although it is also present in the contributions of Craycraft and Keys. Hauerwas warns Catholics not to become too ecumenical. He takes certain "liberal" Catholic (and Protestant) theologians to task for advocating such concepts as "civility" (which is simply an Enlightenment word for "socially appropriate") and "conversation," explaining that Catholics must resist "the forces of modernity that grind all genuine dispute into calm 'conversation' " (p. 228). He insists that we not forget that "Catholics and non-Catholics live in different worlds. That is why Christians do not seek to convince the other; we seek to convert the other" (p. 230). This forms an interesting contrast to Neuhause's claim in the preface that "this book seeks to *convince* us that the principles and practices of a free society are made necessary by Catholic teaching" (p. ix). Are we to conclude from Hauerwas's dismissal of civility and conversation in favor of conversion that Catholicism deserves the mistrust of secular liberals mentioned at the beginning? The conservatism of Hauerwas and Craycraft indicates only that religiously inspired political theory is as diverse and prone to disagreement as any other tradition in political theory—and a good thing, too.

**Feminist Interpretations of Hannah Arendt.** Edited by Bonnie Honig. University Park: Pennsylvania State University Press, 1995. 383p. $55.00 cloth, $18.95 paper.

Jeffrey C. Isaac, *Indiana University, Bloomington*

As Elisabeth Young-Bruehl notes in her blurb on the back of this book, it is "startling" that Hannah Arendt should have become so "provocative" a subject for feminists. For Arendt, as Bonnie Honig observes, is a theorist who has widely been disparaged by feminists, for her indifference to gender issues and, even more important, for her insistence on conceptual boundaries—the private versus the public, the social versus the political, the natural versus the created—that render the politicization of gender difficult if not impossible. And yet Honig and her collaborators have managed to move beyond this negative assessment of Arendt and to construct a series of fruitful encounters between feminist concerns and Arendt's corpus.

As Honig's introduction makes clear, the guiding insight of the volume is that "the feminism question in Arendt" needs to be joined with "the Arendt question in feminism." In short, while feminist theorists, and political theorists more generally, need to come to terms with Arendt's deficiencies as a theorist from the point of view of feminist concerns, feminists need also to rethink the "women's standpoint" from which their criticisms of Arendt typically have issued. From the perspective of a more pluralistic feminism it becomes possible to reread Arendt; indeed it becomes possible to see Arendt's conceptions of agency, plurality, and representative thinking as resources for feminism.

Most of the essays in this volume make a good case for such a rereading of Arendt. While they represent a variety of perspectives, the fourteen essays seem to fall into two categories. Some—like the pieces by Honig, Susan Bickford, Mary Dietz, Lisa Disch, Melissa Orlie, and Linda Zerilli—present critical reappropriations of Arendtian concepts. The general thrust of these essays is to argue that feminism must come to terms with the complex intersections of gender with race, class, sexuality, and political opinion, and that traditional feminist conceptions of female identity, "sisterhood," and political agency need to be refigured along these multiple lines. Arendt, because of her keen sensitivity to the plurality and contingency of human affairs, is viewed as a theoretical ally in this cause in spite of her own indifference to feminist concerns. Many of these essays are influenced by postmodern writing, and Honig's "agonistic" reading in particular reflects the influence of Michel Foucault and Jacques Derrida. But equally important is a common awareness of the political difficulties confronting essentialist feminists who claim to speak on behalf of "women" without attending to the actual plurality of women's experience.

Another group of essays—by Seyla Benhabib, Joan Cocks, Jean Elshtain, Morris Kaplan, Joan Landes, and Hannah Pitkin—present historical contextualizations of Arendt's writings. Each of these essays illuminates some aspect of Arendt. Kaplan, for example, usefully links Arendt's writing on race and anti-Semitism to questions of sexual representation and the marginalization of homosexuality. Landes explores the limitations of Arendt's account of the French Revolution in *On Revolution*. Among these fine essays, Pitkin's "Conformism, Housekeeping, and the Attack of the Blob: The Origins of Hannah Arendt's Concept of the Social" stands out for its brilliant account of the way Arendt's early concern with Jewish assimilationism shaped her conception of "the social" and its dangers.

In a category of its own is Anne Norton's "Heart of Darkness: Africa and African Americans in the Writing of Hannah Arendt." To her credit, of the contributors to this volume, Norton is the most resolutely critical. She believes that Arendt's work fails to grapple with the problems of race in America and, indeed, that Arendt was afflicted by a narrow-mindedness that bordered on racism, and she drives these points home with a vengeance. Yet, her essay is intemperate and poorly argued, making its case through misleading and partial quotation and by ignoring the very different contexts in which Arendt commented on questions of race. Norton's method reaches absurdity when, after condemning Arendt for her supposedly negative stereotypes of black people, she finds fault with Arendt's famous remark that African slavery was the United States' "original crime" because this comment occludes the oppression of women and other minorities! Arendt, it seems, can do no right as far as Norton is concerned. To be fair, Norton's essay does raise important and disturbing questions about Arendt's thinking about race in America. There is no doubt that Arendt's way of writing about this issue is disturbing to our post-sixties sensibilities, and I think there can also be no doubt that Arendt's thinking was narrow and her judgments sometimes mistaken. An excellent essay remains to be written on the way Arendt's thoughts about race were shaped by the heady and racially fractious events of the late 1960s—the New York City teacher's strike and the movement for community control in Ocean-Hill Brownsville, with its sometimes anti-Semitic overtones, the convergence of left-wing Third Worldism and opposition to Israel in the post-1967 period, the genuinely intimidating posture of some forms of Black Power advocacy, and the way these activities frightened white college teachers. But Norton's essay is not that essay. It treats these matters, and Arendt's comments on them, much too cavalierly, without any of the sense of "agonism" and complexity that is so ably developed in the volume's other essays.

Norton's essay is usefully compared with Elshtain's "Political Children." Both begin with Arendt's famous, controversial, and mistaken criticism of federally enforced school desegregation in Little Rock, Arkansas. Yet, while Norton is intent on prosecuting Arendt for the crime of racism, Elshtain asks what sense can be discerned in Arendt's strictures against the politicization of children. Her conclusion is not uncritical of Arendt. But instead of self-righteously condemning her, Elshtain seeks to learn from her argument. The result is an interesting exegesis of Arendt and, more important, a perceptive and intelligent commentary on a wide range of political movements, from civil rights to anti-abortion, and on a matter of genuine political perplexity.

It is this spirit of critical reappropriation that distinguishes the volume as a whole and that makes it an important contribution to Arendt scholarship and to political theory more generally. It might still be argued that there is something strange about turning to Arendt for feminist insights, but I think that most of the authors meet this challenge through their own example, and there can be no gainsaying the fact that this impressive group of feminist theorists *has* learned from Arendt. If I have one criticism of the book, it is that the feminism that it presents is highly theoreticized and makes few forays into the world of difficult political choices and challenges to which so many of the essays allude. (Orlie's essay is a refreshing exception in this regard.) But it is, after all, a book of theory; and it demonstrates through argument and example that feminist theory is a vital enterprise, raising important questions and helping to reshape our understanding of the discipline and its "founders."

**The Barbarism of Reason: Max Weber and the Twilight of Enlightenment.** Edited by Asher Horowitz and Terry Maley. Toronto: University of Toronto Press, 1994. 312p. $60.00 cloth, $25.95 paper.

David Sciulli, *Texas A&M University*

Max Weber has left theorists today with two major problems pitched at two different levels of analysis. One problem, at an institutional level of analysis, is this: How is it that democratic social orders remain present today despite all of the systemic pressures to the contrary? Theorists who address this problem study "civil society" and, in particular, the intermediary associations (professions, corporations) that mediate the state's power and individuals' behavior. The other problem that Weber could not resolve falls at an individual level of analysis: What does it mean to be a responsible individual in the midst of public and private bureaucracies?

The contributions to this collection edited by Asher Horowitz and Terry Maley revolve around the second issue. The editors set the tone (chap. 1) by noting that since 1980 exegetes have focused on Weber's vision of renewing the individual in a world of "disenchantment" and declining meaning. This vision of heroic individuality is the only escape route that Weber left open in the face of the systemic pressures that he saw leading modern societies toward authoritarianism, the "iron cage."

Christian Lenhardt (chap. 2) anchors the volume by showing that Weber followed Kant in privileging individuals over groups or corporate entities. The problem that Lenhardt sees with Weber's subjectivism is that it is not counterbalanced by Kant's "grounding" against relativism: a universal, substantive ethics (e.g., the categorical imperative). Instead, Weber adopted a Nietzschean stance, namely, that individuals' values are invariably pluralistic and thereby competing. Moreover, Lenhardt believes that Weber adopted this stance in a rather cavalier way: "What surprises the reader more than anything is that the skeptical-irrational turn in Weber's value theory comes about unreflectively, without forewarning, certainly without awareness on his part of the import of what he is doing" (p. 37).

Weber's "perspectivism" means that he lacks the concepts with which to defend parliamentary democracy with reasons, as opposed simply to favoring it personally. Lenhardt tries to help Weber out by citing Rickert's view that people (somehow) share certain values and that this ready consensus gives these values an "objectivity," a grounding against relativism (pp. 43–5). However, one point to consider (from Tocqueville, then Parsons) is that if people really value parliamentary democracy, then the evidence for this will be found in how managers and administrators behave, in how they exercise collective power in intermediary associations, not simply in what they (and others) say they believe in survey research.

Fred Dallmayr (chap. 3) explores how Weber, unlike Hegel, treated the state's legitimacy as a strictly subjective, thereby relative, matter (p. 55). Dallmayr appreciates that Jurgen Habermas's turn to the procedural norms of intersubjective communication takes us some steps back to Hegel's grounding, but without recapitulating Hegel's "essentialism" (or more substantive normative approach) (pp. 63–5). Still, Dallmayr is skeptical because Habermas's social base for intersubjective communication, the "life-world," presupposes "an already shared world" of meaning (p. 65). Thus, Habermas ends up being similar to Rickert by asserting that individuals share certain values.

Mark Warren (chap. 4) addresses the relationship between reason and power in Nietzsche and Weber (and presents a helpful figure [p. 71] that helps readers visualize Weber's advance). More clearly than the other contributors, Warren demonstrates that Weber and Nietzsche both saw resistance to domination coming solely from heroic individuals, not from intermediary associations that institutionalize normative mediations on collective power. Warren attempts to salvage constitutional law as a nondominating source of institutionalized reason, but he considers only how constitutions frame interpersonal behavior, not how they mediate public and private exercises of collective power (p. 88).

The Warren chapter leads directly to David Beetham's opening question (chap. 5, reprinted from 1989): Was Weber a critical analyst of liberalism or a critic of liberalism? Beetham finds neither view adequate, because Weber redefined liberalism itself. He abandoned the (purported) universalism of the Anglo-American natural rights tradition in favor of treating the liberal individual as situated in time and place without such normative support (pp. 102–4). In turn, instead of seeing most people as being capable of self-development (*Bildung*, or cultivation), Weber followed German tradition in seeing a select set of individuals retaining this potential (see Ringer 1969, *The Decline of the German Mandarins*). Ultimately, Weber hinged the future of liberalism on the rise of leaders who, once elected, act outside any and all institutionalized restrictions (including popular opinion). Curiously, Beetham ends his article by lambasting Margaret Thatcher as a "plebiscitary leader of Weberian hue" (p. 111), yet nowhere in his chapter or citations does he offer an alternative to Weber's notion of the heroic individual saving parliamentary democracy.

Tracy Strong (chap. 6) builds on Beetham's analysis by exploring what Weber thought it meant to be bourgeois under modern conditions. In particular, Weber asked by what standard individuals can judge their own behavior, as knowledge becomes strictly instrumental (p. 114). Not seeing any common normative standard of evaluation (p. 121), Weber concluded that individuals must seek their bearings "from the quality or character of a person of knowledge." Strong's argument is that Weber saw the bourgeois of early capitalism having "the right to make claims about the world" (p. 124). The bourgeois's "trained relentlessness," his effort to act responsibly in the midst of social life's "irreconcilable fragments," gives him the credibility to make claims, whether in science or in politics (pp. 129, 126). Once again Weber leaves us with the heroic individual bucking the tide, unsupported by intermediary associations of any kind.

Following Terry Maley's (chap. 7) point that Weber treats as self-evident that he (and the cultivated) know what heroic action means (p. 158), Raymond Morrow (chap. 8) explores how Mannheim and the Frankfurt School reacted to Weber's position. Mannheim saw the intelligentsia being capable of transcending competing values by developing a synthetic vision informed by social theory (p. 178). Habermas eventually follows Mannheim, supplementing his turn to the intelligentsia with his own study of a public realm in which reasoned ideas may prevail over unreasoned interests.

Asher Horowitz (chap. 9) continues the discussion of Habermas's response to Weber. One of Habermas's insights beyond Weber is that developed generations before Weber by Anglo-American liberal and constitutional theorists: The rationalization of law (and then, presumably, of courts) need not invariably contribute to domination. Yet Horowitz correctly finds that Habermas ends up tying the fate of intersubjective understanding (and communication ethics) to the presence of ideally socialized individuals (p. 210), even as Horowitz rejects the idea that this simply leads us back to Weber's heroic individualism (p. 213).

Following Alkis Kontos's masterful, elegantly written discussion (chap. 10) of the *enchanted* world against which Weber constructed his view of rationalization and disenchantment, Gilbert Germain (chap. 11, reprinted from 1993) offers a compelling argument for treating both the capitalist economy and the larger social order "as a system" that is open-ended, not fated to enter an iron cage (p. 249). Where Weber's modern world is populated by individuals, systems are animated by institutions: "One would assume that individual actors are generally less likely than institutions—that is, businesses, bureaucracies, governments, etc.—to take advantage of technically correct knowledge in their deliberations" (p. 252). With this observation, Kontos opens the way for students of Weber to move to the sociology of organizations and then potentially to a critical theory of intermediary associations.

Susan Hekman (chap. 12) confronts directly the relationship between Weber's subjectivism and today's postmodernism, as initiated by Foucault. In one of the best discussions in the literature of Weber's ideal type approach, Hekman explores the tensions that Weber experienced in trying to defend "objectivity" and "value-neutrality" despite his own fidelity to Nietzsche's perspectivism and his own political commitments. She appreciates that Weber's heroic individualism is vulnerable to Foucault's skepticism.

Finally, Sheldon Wolin (chap. 13, reprinted from 1981) argues persuasively that Weber's methodology is a political theory of sorts, a theory of a founding coupled with a critique of rival contenders. Nevertheless, the "fundamental political riddle," as Wolin correctly sees it, was not exactly the issue in the forefront of Weber's concerns, whether in politics or in science: "How to combine vast power with perfect right" (p. 289). Wolin notes that Weber inherited a situation wherein "science appeared to be power without right" and "science was acknowledging that, by nature, it was incapable of supplying the missing component of rightness" (p. 290). While he wished to address this legitimation crisis in science, "Weber never set down a coherent political theory" (p. 291). He offered the social sciences a methodology instead (p. 292). Because Wolin sees this as typical in the social sciences today, he calls social science "the post-modern form of political theory" (p. 301).

Overall this collection is a valuable resource not only for Weber specialists in particular but also for theoretically inclined political scientists and sociologists. I say this even as I am not one who is particularly enamored of ever more exegeses of the classics. This collection demonstrates, in my view, that Weber, like other major social theorists and philosophers of his day, did not leave us with the concepts that we need to answer the institutional problem I posed at the outset: How is it that democratic social orders are sustained today despite all of the systemic forces to the contrary? Given this collection's many contributions in substance, one drawback in form is that it lacks an index.

**Rights.** By Peter Jones. New York: St. Martin's Press, 1994. 258p. $49.50.

Jack Donnelly, *University of Denver*

Peter Jones's *Rights* provides a careful and wide-ranging review and assessment of theoretical work on rights, emphasizing human rights. It is one of those rare introductory surveys that is thoroughly accessible to good undergraduates, yet intelligent and probing enough to engage scholars who have read fairly widely in the field.

The first three chapters provide a general conceptual orientation. Chapter 1 sets out Wesley Hohfeld's classic categorization of types or forms of rights. Jones focuses on the differences between claim-rights and liberties, but also provides useful discussions of powers and immunities. Chapter 2 reviews the major contemporary Anglo-American theories of legal rights, giving special attention to theories that emphasize benefits, choice, and entitlement (which is Jones's own preferred account). The chapter also provides a useful discussion of the relationship between rights and sanctions. Chapter 3, which turns to moral rights, contains an excellent discussion of utilitarianism, undertaken from a largely critical, rights-based perspective. Jones also provides a suggestive account of the limits of purely rights-based theories, along with a useful analysis of the kinds of beings that can hold moral rights.

Chapter 4 moves from rights in general to human rights in particular. Jones's discussion of Hobbes and Locke is solid and informative, making good use of the distinction between liberties and claim rights. The sketch of selected basic conceptual features of human rights, however, is somewhat more scattered, and I did not find the discussion of the distinction between natural and human rights particularly penetrating or engaging. One has the sense that this is material that must be worked through in order to get to the more interesting issues considered in the remainder of the book.

Chapter 5 provides pointed yet fair reviews of major contemporary theories of human rights, especially those of Rawls, Nozick, Gewirth, and Finnis. Readers who want a short, balanced overview of recent theoretical work can do no better than this. Personally, however, I was more interested in the briefer discussion at the end of the chapter on pluralist accounts of human rights. Jones suggests that human rights may be capable of multiple, not necessarily compatible, theoretical justifications. This raises the interesting possibility of trying to apply Rawls's notion of overlapping consensus in this context. Also intriguing is his suggestion that "if we recognize many sorts of value and if we do not accept that those many sorts of value can be reduced to a single value, it may be that different sorts of right[s] stem from different sorts of value. . . . We should not therefore take for granted that an investigation into the foundations of rights must be an attempt to unearth a single and all-encompassing 'grand' theory of rights" (p. 118).

Chapters 6–8 cover freedom, socioeconomic rights, and democracy. In his account of rights to liberty, Jones emphasizes the value of autonomy, with special attention to Kant. (In passing, I would suggest that Jones errs in focusing exclusively on Kant's discussion of autonomy in his moral writings and ignoring the discussion in his political writings.) He also gives attention to instrumental or consequentialist arguments for autonomy, especially those of Mill. Jones makes the useful point that "the unsatisfactory nature of unqualified consequentialism should not prevent our recognizing that some of the strongest arguments for upholding rights to specific freedoms *are* instrumental in character" (p. 138). This is reflective of his unusually open- and fair-minded approach and of his appreciation of the complexity and variety of issues encompassed within the general topic of rights.

Jones is sympathetic to the view that economic, social, and cultural rights might be better conceived of as rights of citizens than as human rights. The chapter, however, does include valuable considerations of the relationship between needs and rights, and rights and resources, and of Cranston's well-known arguments against economic, social, and cultural rights. The discussion of democracy in chapter 8 usefully emphasizes differences between human rights and democracy as well as their links. For Americans, who often unreflectively see the two as equivalent, this may be particularly useful.

Chapter 9 and the conclusion turn to problems for (human) rights theories. Jones gives particular attention to defeasibility, cultural relativity, and disputes over rights. And although he emphasizes the compatibility of rights and community, in response to an increasingly popular line of criticism, Jones also stresses the fact that rights represent only one part of moral reasoning. The book is supplemented by an excellent essay that guides the reader through the extensive bibliography.

Jones presents the views of others fairly while developing a subtle running argument of his own on the character and importance of rights. He makes unusually effective use of conceptual distinctions developed in the early chapters to clarify substantive issues that arise in later chapters. And he refuses to try to resolve substantive moral issues by recourse to conceptual or linguistic devices. For example, he notes that "a certain amount of 'specialness' is built into the very idea of rights, but the exact moral status that should be given to any particular right is a moral, not a conceptual, question and we should expect people to answer it differently" (p. 193). In Jones's hands, conceptual analysis clarifies issues and disposes of certain arguments, but wisely leaves many of the most important questions open for resolution by substantive moral argument.

This book admirably achieves its relatively limited, but significant, aspirations. It is, in my view, the best starting point available today for anyone who wants a serious theoretical introduction to rights and human rights.

**The Handbook of Experimental Economics.** Edited by John H. Kagel and Alvin E. Roth. Princeton: Princeton University Press, 1995. 721p. $55.00.

Kenneth C. Williams, *Michigan State University*

This volume, edited by John H. Kagel and Alvin E. Roth, contains an impressive set of essays that summarize the present state of experimental economics. In light of the current controversies in political science concerning the empirical testability of formal models, it illustrates the merits of using laboratory experiments to test theories when there is a lack of naturally occurring data or when the available data are tenuous at best. Although the use of experimental methods among political scientists to test formal models has increased in recent years, it still has not attained the nearly mainstream status it has in economics. This book demonstrates how laboratory experiments have progressed from being a fringe methodology to a reasonable way of not only testing but also exploring economic theory. It provides accounts of how laboratory experiments have confirmed existing theoretical predictions, helped decipher puzzles that have baffled researchers for years, and led researchers to investigate different aspects of a theory that would have gone unnoticed without experimentation.

Each chapter focuses on a series of experiments within a given topic area. This approach is particularly useful since it allows the reader to trace the progression of the experimental process revealing how hypotheses are formulated and reformulated into new experimental designs. As a result, the authors not only provide compelling evidence for the usefulness of laboratory experiments but also do not mask their inadequacies. The authors make it clear that laboratory experiments are not a substitute for naturally occurring data; rather, when there is a sparsity of relevant data, experiments provide a reasonably good mechanism to test parts of a theory under special conditions. Each author is cautious about not overgeneralizing results obtained from the laboratory. They realize that experiments test a particular hypothesis. Consequently, the environment that is created often has no real-world counterpart; or if a counterpart does exist, the experimental environment is a gross simplification of it. In addition, experimental data may be biased, since the rewards offered to subjects may not be sufficient to control their incentives. A more fundamental problem is that experiments are often conducted in different environments, making it difficult to make comparisons across sets of experiments. Given these shortcomings, however, it is clear from this volume that there are research questions that cannot be answered without the use of experimental data. We learn that carefully designed laboratory experiments can yield fruitful results.

While this volume concentrates primarily on issues relevant to economists, most of the topics should be of interest to political scientists as well. Besides the introduction, of particular interest are the chapters by John O. Ledyard on public goods, Jack Ochs on coordination problems, Roth on bargaining, and Colin Camerer on individual decision making. While the chapters by Charles A. Holt on industrial organizations, Shyam Sunder on experimental asset markets, and Kagel on experimental auctions are excellent, they may be beyond the interest of some political scientists. However, Sunder's chapter is a must-read for researchers interested in how various environments help to aggregate information.

The introduction to this volume is splendidly done. It gives the reader an adequate history of experimental economics, tracing its progress from its roots in the individual choice experiments of the 1930s up to the present. The introduction also provides an excellent overview of the major methodological issues that are discussed in subsequent chapters.

Experiments that seek to study behavior in voluntary contribution mechanisms are surveyed by Ledyard. Of interest are variables that lead subjects to contribute to a public good when it is in their self-interest not to contribute. Experiments conducted by sociologists, social psychologists, political scientists, and economists are discussed. Data reveal that even though economic theory suggests that subjects should not contribute, they generally do in the laboratory. Even in environments conducive to noncooperative behavior, the rates of contributions cannot be forced below 10%. Experimenters seek to determine which factors alter the relative rates of cooperation. There are strong findings that show that cooperative behavior improves when communication is permitted and when the marginal payoff for contributing increases. Repetition of play generally causes contributions to decline. Other factors may be relevant, but the evidence is inconclusive.

Ochs's chapter focuses on macroeconomic experiments that examine how subjects coordinate strategies when there are multiple equilibria to a game. Experiments are conducted to understand how different mechanisms (e.g., the existence of dominated strategies) and environments (e.g., overlapping generations and decentralized matching) allow subjects to coordinate actions. This series of experiments illustrates an important characteristic of economic experiments—repetition. Experimenters in this series are not concerned about whether in the early rounds subjects identify and select an equilibrium. Rather, they examine behavior over time to determine if subjects learn to select strategies that converge to a predicted equilibrium. Convergence occurs in most cases in experiments that specify a unique equilibrium; when multiple equilibria are specified, only experienced subjects tend to coordinate on one of them. Results suggest that different theories of "strategic" learning may better explain behavior.

Inefficiencies in bargaining experiments are the focus of Roth's chapter. Data from sequential bargaining experiments, in which players make alternating offers, suggest inefficient outcomes may be a by-product of uncontrollable effects. For example, some deviations from the predicted outcome may result because bargainers often have nonmonetary incentives not specified by the theory. Additional studies have attempted to control for incentives such as concerns over relative shares and notions of fairness. Other experiments reported by Roth suggest that experience and learning are important determinants of bargaining outcomes. As with coordination, further learning theories need to be studied. Finally, as revealed in the public goods experiments, bargaining in the laboratory is also affected by controls over communication.

Camerer surveys individual decision-making experiments and in the process provides an excellent contrast between psychological and economic experiments. Experiments of this nature attempt to determine whether subjects in the laboratory emulate the assumptions of rationality. Methodological differences between the two disciplines seem to account for varying results. Economic experiments, which rely on repeated play, monetary incentives, and anonymous interactions, tend to provide evidence that subjects' actions are generally consistent with the assumptions of rationality. By contrast, psychological experiments, which often do not provide monetary incentives, do not allow repeated choices, and sometimes deceive subjects, find evidence to the contrary. Camerer points out, however, that different results between the two disciplines are directly related to disparate research questions.

Overall, this volume gives the reader a greater appreciation for laboratory experiments as an integral part of theory confirmation and development. While the laboratory is not flawless at present, with a proliferation of formal models, it is an invaluable research tool. This book is excellent in displaying the methodology's strong points and its shortcomings. Each chapter leaves the reader with more questions than answers, thus providing material for a new volume in the near future.

**Leo Strauss and Nietzsche.** By Laurence Lampert. Chicago: University of Chicago Press, 1996. 239p. $22.50.

Edward Andrew, *University of Toronto*

Laurence Lampert has shared a Dionysian communion cup with Nietzsche; *Leo Strauss and Nietzsche* is the result of this pious intoxication. It is written with confidence and grace, and without argument or judgment. The thesis of the book is that Plato, Nietzsche, and Strauss were atheists who thought philosophers had to play the role vacated by the dead gods. The fundamental intention of Plato, Nietzsche, and Strauss was to rule the world (pp. 128–9). Lampert seems to address only the growing number of American academics who already hold Nietzsche's teaching to be compelling and specifically calls upon Straussians to come out of the closet and avow their hidden Nietzscheanism. There is little argument for those of us who are neither Nietzscheans or Straussians. If, as Zarathustra said, the old gods died of laughter at hearing the jealous God declare that only He existed, Lampert's claim that the only genuine philosophy is that of philosophic legislator-commanders might be intended to kill off us non-Nietzscheans.

Lampert accepts the argument of Shadia Drury's (1988) *The Political Ideas of Leo Strauss* that Strauss was a Nietzschean (pp. 132–3), although he distances himself from Drury's defence of natural law. Leon Craig's recent (1995) *The War Lover* also conflates Plato's, Nietzsche's, and Strauss's teaching. Against the apparent position of Strauss that Plato thought philosophy to fly with the wings of Eros, not with those of the Nietzschean eagle, Craig imputes to Plato Nietzsche's view that philosophy is an activity of the warrior spirit, while Lampert assimilates Nietzsche to Plato by perceiving the will to power to be fundamentally an erotic longing for eternity (pp. 54–6).

While Lampert is doubtless right to think Nietzsche was a major influence on Strauss, Lampert's dismissiveness of Heidegger and Heidegger's *Nietzsche* perhaps gives a one-sidedly Nietzschean cast to Strauss's reformulation of Platonic political philosophy. For example, Strauss did not, to my knowledge, write about the creation of values—except when paraphrasing Nietzsche—whereas Lampert constantly does. Curiously, in the course of showing the compatibility of value-creation and Nietzschean naturalism, Lampert states that "creativity for its own sake counts for less than nothing in Nietzsche, *less* than nothing because mere creativity is the modern way, the way of the actor, the way Nietzsche most opposes. . ." (p. 98). While Lampert's attempt to demonstrate how one can generate values from nature—will to power as the fundamental natural fact and as the most coherent interpretation—is not without interest, one remains perplexed why Lampert unremittingly uses the Nietzschean language of value-creation, while deprecating mere creativity.

A second difference between Nietzsche and Strauss pertains to the relationship between the assumption of a natural order of rank and political aristocracy. My impression is that Strauss was interested more in the difference between philosophers and nonphilosophers than in the rank of soldiers, financiers, physicists, poets, actresses, bishops, public relations men, whores, and the other 1001 occupations essential to an aristocratic order or rank. Strauss did not seem interested in Nietzsche's views on Roman aristocracy, for example, even when it would seem to be called for, as in his *Thoughts on Machiavelli*. For all Lampert's insistence that we can generate values from the natural order of rank, the only ranking Lampert provides is the superiority of philosophers over poets, and men over women (pp. 110, 155–9). Those of us lusting for the sexist rule of philosophy might still cautiously await the place of militiamen, businessmen, sociologists, athletes, and dancing masters before signing onto this new aristocracy. To expand his aristocratic constituency, Lampert might be encouraged to spell *plebeian* correctly in his diatribes against things plebeian.

Although Aristotle held Plato to be a Pythagorean, Strauss systematically ignored, or deprecated in passing, the Pythagorean dimension of Plato (the doctrine of the forms, the religio-political practices, the equality of women). Strauss did not seem to be very impressed by Plato's epistemology and ontology, or indeed those of any other philosopher. The Nietzschean antithesis of noble/base seems to have replaced the Platonic true/false in Straussian thought. Does that mean that Strauss believed that all fundamental thinking is a form of willing, evaluating, or estimating in terms of rank? Was Strauss so sceptical of truth claims that the noble/base dichotomy served to hide the perceived fragility and vulnerability of the philosophic quest, the fear that the harbor of truth does not exist and that no known anchors can reach the bottom of our stormy seas? Was Strauss so tossed in our stormy time that, when blown to the land of Oz, he discovered the philosophic wizard to be an ordinary little man but, hiding his discovery, proclaimed to his followers that the yellow brick road was the golden path to wizardry?

Lampert's Strauss presents philosophers as wizards. All history is the history of philosophy. Plato's defensive stance against Aristophanes' charge that philosophy is atheistic engendered Christendom, "Machiavelli successfully masterminded" the end of the medieval enlightenment, Bacon and Descartes founded our scientific-technological world, Nietzsche's thought of eternal return will restore nobility if not divinity to our godless time, and Strauss "made himself an indispensable guide to world-making philosophers." The fundamental question for Lampert is why Strauss took pains to disguise his Nietzscheanism, rather than overtly espousing an atheist credo of world creation. Lampert does not consider the practical difficulty of a Jew, before and after World War II, proclaiming himself the anti-Christ or the prophet of new pagan values; our Nietzschean age, Lampert asserts (p. 15), "does not depend upon our hushing it up that God is dead or that philosophy corrupts." After quoting Strauss's denunciation of Nietzsche's advocacy of the "merciless extinction" of large masses of men, Lampert denies that the quoted words come from Nietzsche and then goes on to say that Nietzsche was not an anti-Semite and would not have supported Nazi crimes of merciless extinction (pp. 8–9). This denial seems to, but actually does not, deny the many passages where Nietzsche calls for the elimination of large numbers of people who are not useful to his project of creating a new aristocracy. Thus Lampert does not address one good reason for Straussian esotericism about his alleged Nietzscheanism, and may even give reason to doubt Lampert's much-proclaimed exoteric Nietzscheanism. Lampert champions not only Nietzsche's natural nobility in scorning Christian morality but also his great strategic sense, which Strauss lacked, in not keeping up a facade of piety in an age where the God business is no longer profitable. Lampert calls on his fellow Straussians to abandon petty national politics, the common calling of classroom teaching, and the incentive of research grants from right-wing agencies, for the grand Nietzschean politics of world-creation. *Leo Strauss and Nietzsche* is a provocative and readable book, written with clarity, energy, and style. It is to be hoped that it will provoke some Straussians to thought.

**Paradoxes of Gender.** By Judith Lorber. New Haven: Yale University Press, 1996. 424p. $35.00 cloth, $15.00 paper.

Terrell Carver, *University of Bristol* (UK)

Lorber constructs an analytically clear, sociologically wide-ranging, and politically pertinent account of gender. Arguing from a thoroughgoing social constructionist position and closely following Judith Butler and Donna Haraway, she declares that "procreation and sexuality are constructed as conditions of subordination within the social institution of gender" and that gender is therefore not rooted in procreative difference and sexual orientation (p. 285). Using methods of feminist deconstruction (p. 5), she has found even more numerical varieties in the current "understanding" of gender than I have (e.g., five sexes, three orientations, five displays, six relationships, ten self-identifications, infinitely varied practices, and four sexual statuses) (p. 59). Citing widely from a vast literature, Lorber is able to cover almost any issue to do with power differentials based on sexual difference that one could think of.

One of the strengths of the book is the way that successive and different kinds of feminist theorizations (liberal, radical, Marxist, cultural) are acknowledged and recouped—without ponderous chronologies and overfamiliar narrative. Competing, complementary, and contradictory analyses of, say, women's subordination and hegemonic masculinity are deployed for the reader in a manner that neither fuzzes over the author's convictions nor presumes on the reader's discrimination. While much of the focus is on women, as subordinates in (almost) all gender systems, the category itself is always presented as cross-cut by race/ethnicity, class, and other ascribed characteristics in social hierarchies. The pluralization of masculinities, the issues raised by gay studies, and even the "what is sex, anyway" problems raised by sadomasochism, for example, are duly covered. Queer politics, however, does not get a look-in, though gender-bending, liberating bathrooms, and multiperson households/shared parenting are all mentioned. In short, the author writes from a clear and forthright perspective, deals sympathetically and very widely with a huge variety of issues, and argues knowledgeably from within a considerable number of disciplinary perspectives (history, cultural studies, women's studies, feminist theory, empirical sociology, anthropology, the psychoanalytic tradition, Marxism, men's studies, etc.).

One of the weaknesses of the book is probably now apparent. Its clarity, concision, comprehensiveness, reliability, and sheer readability as a survey—all very admirable, and a significant "other" to the well-known genre of academic obscurantism—are purchased, apparently, at the price of anger, humor, passionate engagement, worry, laughs, and—dare I say it—*jouissance*, or anyway, the kind of academic writing that zings with transgression. Butler and Haraway did that for me, and I guess they did it also for Lorber, but I get the feeling that the real paradox of gender here is the way that Lorber's book does not seem very troubling and is hardly a manifesto. Still, I plan to recommend it widely and put it on reading lists.

One reason for doing this is Lorber's persuasive use of plurality. She manages to jump about in one paragraph from the United States in World War II, "Islamic countries that have become more fundamentalist," and postcommunist economies in Eastern Europe (p. 9)—and get away with it—because her point is not that gender hierarchy is the same in different times and different places, but that in different times and different places it is changeable and has changed. And in those situations she also catches the ambiguities and compromises of women's politics as women undertake it. There are similar brief studies for other issues and other groups in the gendered world—sexual violence, employment, parenting, sexual commodification, civil rights, and equality issues.

On equality and equity Lorber's argument is exceptionally clear and useful (though she is hardly the first to make it): In a social structure that creates hierarchies that follow, not just from procreative difference (irrespective of individual variation), but from an already hierarchized concept of just what that difference is, equality between men and women (leaving other groups aside) will never really occur, either as a strategy of treating men and women in the same way (where they can be presumed to be the same) or as strategies of compensation (where they can be presumed to be different) (pp. 282–5). Lorber gets MacKinnon at her best in this argument and gives due respect to liberal goals—another instance where generosity and eclecticism advance the argument.

Courageously, Lorber puts an important argument in characteristically direct and simple terms: "Human beings have constructed and used gender—human beings can deconstruct and stop using gender" (p. 297). In her concluding pages she sketches a world in which bodily differences and differences in sexualities would be visible and would matter, but would not be used to create hierarchies of power and disesteem. Moreover, other hierarchies of power, notably those to do with paid and unpaid labor as well as racial/ethnic and similar sorts of discrimination, would have to be tackled simultaneously, so she argues.

Excluding violence, but including fantasies, play, and (even) pornography (as "libidinally satisfying"), Lorber's egalitarian utopia is stronger in imagining parenting and domestic labor conducted in ways that are attentive to individual difference yet ungendered, and hence equal between the sexes, than it is on the macro- and microeconomics of a (presumably high-tech) world in which "different talents and contributions are equally valued and rewarded," and all have "equal access to the valued resources" of society (pp. 294–7). Still, that hardly invalidates her train of thought or renders nugatory her attempt, but she did not quite say about capitalism what she said about gender. David T. Evans's (1993) *Sexual Citizenship*, not cited by Lorber, comes close to doing this, but I am sorry to report that his is a pretty pessimistic account of the problem. On a more optimistic note, one final reason for recommending Lorber's book is that, if anything can, it will put paid to the current redundant and reactionary practice of using gender as a synonym for sex, where sex is represented as the binary M/F. Gender is much too significant an idea for that.

**Pluralism, Justice, and Equality.** Edited by David Miller and Michael Walzer. Oxford: Oxford University Press, 1995. 307p. $19.95.

Avigail I. Eisenberg, *University of British Columbia*

In this volume, eleven leading political theorists evaluate Michael Walzer's notion of complex equality and democratic citizenship in *Spheres of Justice* (1983). Their criticisms fall into three categories. First, Walzer's argument that goods ought to be distributed according to principles derived from the social meaning of the goods does not necessarily yield egalitarian outcomes. Second, Walzer's approach is empirically weak and some of his claims are empirically suspect. Third, Walzer's approach might hinder progressive change because it requires that the principles belonging to one sphere be blocked from influencing the distributions in other spheres. Except for Walzer's responding essay, the collection offers an excellent display of the talents of the contributors. In making their criticisms, the authors provide a good discussion of their concerns related to justice and equality. But, despite the rich role that pluralism plays in democratic politics and its usefulness as an analytic tool of democratic theory, the pluralist aspect of liberal politics is hardly explored at all.

Walzer's notion that distributive principles ought to be derived from the social meaning of a good is the subject of the broadest and most serious criticisms. Joe Carens describes Walzer's work as relativistic and along with Jon Elster argues that there is something amiss in a theory of justice that requires that only local understandings of repression and injustice count as authentic. Richard Arneson argues that what follows from the social meanings approach to distributive justice is that so-called complex equality actually "permits any degree whatsoever of inequality" (p. 239). Taking social meanings seriously compels us to regard as just those instances of domination that are part of a sphere's social meanings—for example, domination in the family under patriarchy or in employment under feudalism. In addition, because spheres often overlap, a multitude of principles of distribution are relevant within each. As Amy Gutmann argues, claiming that one can choose between principles by looking for the "real" social meaning of goods such as education, health care, or employment begs the question of how to detect what the real social meaning is. Contending principles are found within a sphere precisely because its social meaning is in dispute.

The second kind of criticism is that although Walzer wants to base his theory on local understandings and traditions, it does not bear the weight of empirical scrutiny. Drawing from empirical studies in sociology, David Miller argues that complex equality fares poorly in relation to status congruence theory. But Adam Swift finds that it fares better in light of stratification theory. Elster's essay goes the farthest in offering a taste for what would be required of a theory of justice that seeks empirical foundations. He finds Walzer's theory wanting as both a description of what Americans think is just and an explanation of what causes people to think a certain distribution is just. The profound nature of disagreement to which Elster points is well illustrated within this collection of essays with respect to the positions taken on how medical care ought to be distributed. Gutmann argues that individual responsibility ought to count as one of the principles by which medical care is distributed. On this basis, she (mistakenly, according to Walzer) opposes its egalitarian distribution. Thinking through the implications of their exchange impresses upon the reader the *detailed* agreement required by Walzer's theory about the social meanings and principles that ought to govern distribution in each sphere. Even a minor disagreement between relatively like-minded professional theorists of liberal democracy about the role of individual responsibility can translate into a huge difference in the practice of distributing health care and in the personal welfare of millions of people.

The third kind of criticism responds to Walzer's notion of blocked exchanges. He argues that when distributive principles suited to one sphere cross boundaries and set standards of distribution in other spheres certain goods, such as money, inappropriately come to dominate all spheres. With specific reference to the nature of money and the market, Jeremy Waldron and Brian Barry both argue that it is difficult to imagine how one could stop the commodification of most things given the fluid medium of exchange that money is meant to be. And far from being thoroughly oppressive, the flexibility that money has allowed in relations between people has historically been liberating and beneficial as well. Michael Rustin and Susan Moller Okin each point out that sometimes progressive change in the family or the legal system has required not only that traditions be abandoned but that the principles of one sphere influence the distributions of another. Progress relies upon creative experiments in which principles are imported from different spheres.

Despite the excellence of these essays, the volume lacks a critical discussion of the nature of pluralism and fails to mention the restricted application of the concept in Walzer's work. Carens rightly introduces some of the many problems associated with cultural pluralism and points out that Walzer makes no reference to German or African culture when exploring their historical treatment of minorities. But the underlying point needs to be emphasized, and it needs to be viewed from a domestic, as opposed to an international, context. Different takes on the social meaning of property, medicine, money, education, employment, and love are shaped by different historical experiences, different priorities, legitimate fears, and needs. Such differences coexist within the same society and are legitimate components of a society's history and character. For this reason, the task of sorting through social meanings, let alone choosing one, is enormous. Arneson asks Walzer how he intends to treat a Christian Scientist's position in the debate over the distribution of health care. Similar questions can and should be posed with regard to the Mennonite perspective on education and freedom of religion, the Aboriginal perspective on property rights, and the Québécois and Hispanic communities' perspectives on language rights.

Moreover, pluralism exists along more than one dimension. In addition to a pluralism of spheres (e.g., money, education, health care), there is a pluralism of groups (cultural, religious, linguistic) that live their lives through these spheres and a plurality of perspectives within each group. A taxonomy of pluralisms, similar to Judith Andre's taxonomy of "blocked exchanges," might help readers understand the complexity of liberal societies that this concept highlights. *Spheres*, as its subtitle indicates, is supposed to be "A Defense of Pluralism and Equality." But Walzer fails to address this complexity, the interplay between the different dimensions of pluralism, and the politics of a pluralist society. This makes it even more important that a critical discussion of pluralism be found in this volume.

Contrary to Walzer's argument, merely ensuring that one sphere does not dominate all others (e.g., money turning all goods into commodities) does not protect individuals from many forms of domination. One cultural group could impose its social meanings on all the spheres even while policing the boundaries between them. Similarly, one group could dominate all its members, making dissent unlikely or impossible, though never interfering with the boundaries between spheres. Individual and group rights offer one means of protecting individuals from these forms of domination. Another means is found in pluralism itself and can take its cue from some of what Walzer and other pluralists throughout the century have argued. The equitable distribution of power among a plurality of groups provides contexts in which individuals can develop their capacities and lives and restricts the potential for tyranny to arise. As in Walzer's account, pluralism requires that each community or sphere offer to individuals a different set of principles and priorities. But contrary to Walzer's idea, pluralism requires that individuals use the perspective of one sphere or community to scrutinize the practices of another. By doing so, each context acts as a critical vantage from which individuals can detect oppression and domination in other contexts. For example, my peers offer to me principles by which I critically scrutinize my family life. The principles of public education lead me to question the values of my culture and religion. My religion operates according to principles with which I judge the justness of the distribution of wealth in my community, and so on and on.

Spheres and communities will more likely influence each other in societies in which the broadest type of pluralism is protected, a point whose implications are insufficiently appreciated in this collection. Miller contends that "it would be absurd . . . to recommend increasing the number of spheres" and that "it makes no sense to invent new goods on the grounds that this will then lead to greater pluralism in social distribution generally" (p. 223). Yet, it would seem that the health of democratic life relies on ensuring that diverse ways of life in a liberal-democratic society continue to flourish. This might require that the state pursue an active policy of promoting multiculturalism, recognizing language rights and offering special protection for certain cultural practices. In doing so, pluralism aims at attaining three goals worthwhile to democracy. First, in a strongly pluralist state, individuals can securely enjoy the traditions and local understandings of their communities. Second, protecting pluralism means that no one principle, ideal, or way of life can dominate. Third, the requirement that spheres and communities be open to change—that, to a certain degree, exchanges are not blocked—means that each context can act as a critical vantage from which the practices and principles of the others can be scrutinized. In this way, pluralism provides a way in which oppression can be detected. The contributors to this volume raise arguments that are essential to defending a theory of pluralism. But none, including Walzer's essay, recognizes the resources that pluralism offers.

**Poetic Justice: The Literary Imagination and Public Life.** By Martha Nussbaum. Boston: Beacon Press, 1996. 176p. $20.

Francis A. Beer, *University of Colorado at Boulder*

Literature typically gets short shrift from political scientists. Literature may deal with politics, but it is not science. It is not general or abstract, nor is it orderly, simple, or parsimonious. Occam's razor does not cut here. Literature does not present itself as logical or mathematical. It makes little pretense of systematically reporting the results of rigorous investigation. It need not correspond with an objective view of the real physical world or focus on material observables. It is not subject to empirical validation or falsification. In C. P. Snow's construction, it is the other culture. It is art.

Martha Nussbaum is Professor of Law and Ethics at the University of Chicago where, among other things, she teaches a course on Law and Literature. Her current book presents the other side of this debate as she takes up Walt Whitman's earlier call for "public poetry" in political argument. She identifies her "intellectual adversary" as the "law-and-economics movement," which she personifies in the form of Mr. Gradgrind from Charles Dickens's novel *Hard Times*, a man with an accountant's arithmetic mind and "tabular" perspective (pp. xi–xiii, 1).

Nussbaum believes that "literary imagination" is an important alternative and supplement to the Gradgrind point of view. "Public imagination, an imagination that will steer judges in their judging, legislators in the legislating, policy makers in measuring the quality of life of people near and far . . . is an essential part of both the theory and the practice of citizenship" (pp. 2–4). It has the visionary "ability to imagine nonexistent possibilities, to see one thing as another and one thing in another, to endow a perceived form with complex life" (p. 52).

The literary imagination includes "fancy" as a component of "practical reasoning," contrasted with "utilitarian rational-choice models" and their view of "persons as locations of their respective utilities," with their emphases on "facts" and "calculations," with their four basic assumptions of "commensurability, aggregation, maximizing, and exogenous preferences" (pp. 14, 19). Fancy implies incommensurability, disaggregation, independence, and subjectivity. The flight of fancy opens up a richly textured life-world that includes separateness and agency, privacy and desire, worthiness and wonder.

Nussbaum, like Captain Kirk addressing Mr. Spock, also sees emotions as an essential part of human nature and human politics. A well-rounded understanding of politics must include hope and fear, love and greed, passion and compassion, sympathy and empathy. Finally, as the title suggests, a central focus is on justice and on judges. "Poetic judging" (p. 99) brings a richer interpretation of the meaning of the laws, a richer appreciation of the diverse human conditions to which they must be applied. At the very least, the "poet-judge" (p. 120) should write more readable opinions. At her very best, she might approach the wisdom of Solomon.

The book is short and well written. It presents a partisan and compelling case for literature as a significant, though not exclusive, mode of political analysis. Such literary analysis of politics draws from a rich tradition that includes the philosophy of Paul Ricoeur, the anthropology of Mircea Eliade and Claude Lévi-Strauss, the history of William McNeill. It also flows with contemporary currents of legal theory—for example, the work of James Boyd White. It is consistent with renewed interest and attention being given to narrative form in the psychology of Jerome Bruner, the artificial intelligence of Roger Shank, the public policy analysis of Emery Roe.

The appeal of literature is not simply as the practical insider's guide to political life contained in the *The Last Hurrah* or *Primary Colors*. Other works—for example, *The Confessions of Nat Turner* and *I Know Why the Caged Bird Sings*, *Brave New World* and *1984*—illuminate central political themes of individual and community, freedom and repression, equality and injustice. Literature remains an important resource, available to contemporary political scientists and their students who wish to use it. Its great strength is its construction of a politics with individuals who have rich identities and complex selves, with deep and often-hidden inner lives. It calls backward to a well-stocked library of possible worlds. It leads forward to a more human, humanistic, and humane politics.

**Political Philosophy and the Human Soul: Essays in Honor of Allan Bloom.** Edited by Michael Palmer and Thomas L. Pangle. Lanham, MD: Rowman and Littlefield, 1995. 280p. $34.95.

Fred M. Frohock, *Syracuse University*

Allan Bloom was a man of letters who had the good fortune to reach both a scholarly and a popular audience—but not on a plane of universal goodwill and high consensus. For those just arriving on the planet Earth, Bloom adopted and extended the approaches to political philosophy developed by Leo Strauss. His interpretation and use of political philosophy, as a consequence, is celebrated by other disciples of Strauss but contentious for those in rival philosophical disciplines. The popular reception for Bloom's views on education, distributed in the best-selling book, *The Closing of the American Mind*, astonished his critics and delighted Bloom.

The present volume is a collection of tributes to Bloom by his former students. The essays attest to his high intellect and engaging (and sometimes difficult) personality, mainly with anecdotes and samples of the type of political philosophy that Bloom did so ably and with so much academic controversy. Walter Dannhauser, in a reminiscence that opens the contributions, describes a man of loquacious temperament and close and sustaining friendships, a lover of Plato and Rousseau, a defender of classical thought and teaching methods, and a constant protector of a set of beliefs and interpretive techniques attributed to his mentor and friend, Leo Strauss.

The approaches to political philosophy that identify Bloom as a scholar are endorsed by all of the contributors to this book. The rudiments of these approaches are well known, though their intellectual powers are more complex than critics realize and less persuasive than adherents believe. In simple form (exhibited in Bloom's single essay in the collection and elaborated by Christopher Bruell in his contribution) the relevant claims here are these: Philosophy is an inquiry into being; this inquiry will lead to first causes and principles, including the higher principle of justice, that are generalizable to all conventions; there is a symmetry between the individual soul and the political order, which implies that reform of the state can only be conducted with a simultaneous reform of the individual citizen; the symmetry of individual and polis means that education in philosophy is the starting (and perhaps most important) practice of justice; and study of the great books in philosophy is a requirement of education and for the exercise of authority.

Not included in the canon of works elaborated by the essayists in this anthology are the distinctive historiography and methods for reading and interpreting the classic texts that Strauss taught his students. The essays in the volume are characteristically broad in scope. They address works by Strauss (of course), Plato, Homer, Machiavelli, Cervantes, de Tocqueville, Flaubert, and other texts (including the Hebrew Bible). But no one comments on the keys passed on by Strauss for unlocking texts: no assumption of secret writing, no codes for deciphering hidden meaning, no methods for disclosing tacit intentions in philosophical works. Perhaps the special ways that Strauss and Bloom read at least some texts is no longer worth discussing. It *is* puzzling that none of the essayists (except for Dannhauser) has much to say about Bloom's work. The contributions simply represent the approaches to political philosophy that Bloom used in his cultivation of the Straussian program. (I do not know what hidden meaning there might be in this omission, though it might be explained by the fact that good students have their own agendas and Bloom did not follow his master in producing disciples.)

The absence of skeptics in this volume is fair enough. A tribute requires sympathizers, not critics. But the close affiliation of Bloom and Strauss invites an assessment of the Straussian handling of political philosophy, in part as a condition for evaluating the current testimonies to Allan Bloom. Are the approaches any good for research and teaching?

The reliance on a canon in Western thought, with all the requisite assumptions about traditions and unavoidable elitism (in the form of gentlemen for Strauss), is hostile ground in the intellectual pluralisms of the modern world. The prospect of a canon is energizing in a way. It would simplify things if there were a single set of great books, a manageable collection of truths that can be discovered by reason, an ontology that governs conventions in all times and places, and a form of justice that can order both the soul and the city. But critics point to multiple interpretations of texts and the extreme possibility that texts are artifacts of both author and audience. The likelihood that there is no privileged reading of a text raises plausible doubts about the political philosophy that Allan Bloom practiced. One also can reasonably conclude that human experience has proven to be far more heterogeneous, complex, and robust than anticipated even as recently as a century ago. It might be that no theory of forms, no wisdom from the past, can render current experience intelligible or provide principles to govern practices successfully. It is not striking that the great thinkers of the past used resources primarily from their own times and places rather than from the works of their predecessors? Or that innovative work that we now call great has seemed always to be rethinking experience with agendas for the future and has not been preoccupied with the past?

Reading the works of Allan Bloom from an intellectual location outside the scholarly orientations of Leo Strauss is a mixed experience. The excitement that Bloom brings to the reading of a text is indisputable. At ordinary levels he parries and probes like a skillful prize fighter, and at his best dissects passages with the skills of an experienced surgeon of the abstract. Insights that more ordinary readers never see fill the pages. But a distinction found in works by John Rawls (dismissed by Straussians) is always mentally near: the evaluation of an action in a practice and the evaluation of the practice. Bloom did well in a practice that demands more critical scrutiny than its members permit.

The current collection of essays is a helpful volume that represents well the intellectual commitments that Bloom made his own and the affections of his students for a teacher they apparently revered.

**Metaphysics as Rhetoric: Alfarabi's Summary of Plato's Laws.** By Joshua Parens. Albany: State University of New York Press, 1995. 195p. $54.50 cloth, $17.95 paper.

**Plato's *Statesman*: The Web of Politics.** By Stanley Rosen. New Haven: Yale University Press, 1995. 196p. $25.00.

**The Death of Socrates and the Life of Philosophy.** By Peter J. Ahrensdorf. Albany: State University of New York Press. 238p. $59.50 cloth, $19.95 paper.

Mark J. Lutz, *University of Notre Dame*

Despite a revival of interest in classical political thought, Plato remains under a cloud. Weighty thinkers accuse Plato of having relied on untenable metaphysical dogmas to establish the now-suspect tradition of Western political, moral, and religious thought. By paying unusually close attention to the details of particular Platonic dialogues, the authors of these three books belie this criticism by awakening us to Plato's own profound awareness of the problems of philosophizing about politics and by helping us to discover the authentic roots of classical political philosophy.

Joshua Parens's *Metaphysics as Rhetoric* approaches Platonic political thought through the lens of the medieval Islamic philosopher Alfarabi. Unlike many scholars who have virtually dismissed Alfarabi as a minor neo-Platonist, Parens presents the tenth-century thinker as a genuine Platonist who recognizes that Plato does not derive his politics from his theology and metaphysics, but rather derives his theology and metaphysics from his politics. Doubting the availability of metaphysical knowledge, Alfarabi's Plato inquires into political things "phenomenologically," as they present themselves through "commonly accepted opinions" (p. xv). Based on this "dialectical" examination of his interlocutors' beliefs, Alfarabi's Plato develops a "psychology," that is, an account of both the soul's deepest needs as well as of the obstacles that commonly prevent us from meeting those needs, such as fear, shame, indignation, and hope. According to Parens, Alfarabi's Plato ranks political societies according to their capacity to meet those needs and mitigate those obstacles. The particular religious and metaphysical dogmas outlined in Plato's *Laws* are for the most part rhetorical supports for the best political order (p. 144).

Parens's masterful analysis of earlier scholarship on Alfarabi readily demonstrates the need to rethink the prevailing view of Alfarabi's political teaching. Parens makes an especially persuasive argument for reading Alfarabi's "Summary" of Plato's *Laws* on its own, rather than in the context of Alfarabi's other writings, in the manner of Miriam Galston's *Politics and Excellence: The Political Philosophy of Alfarabi* (1990). The only book-length treatment of the "Summary," Parens's provocative interpretation of Alfarabi's teaching should help engender a lively debate among not only medieval but also classical scholars. By tracing how Alfarabi modifies his presentation of the argument of Plato's *Laws* in order to comment on Islamic politics and theology, Parens's book offers an opportunity to rethink the character and purpose of much of medieval political philosophy. As a contribution to the study of Plato's *Laws*, Parens's book is more penetrating than R. F. Stalley's *An Introduction to Plato's Laws* (1983), more accessible and thematic than Leo Strauss's *Argument and Action of Plato's Laws* (1975), and in its own right a useful addition to the astonishingly sparse scholarship on that immensely important work of political philosophy.

Because of the scope of Parens's project and the boldness of his conclusions, some readers are bound to respond with spirited questions and objections. By exploring Plato's teaching thematically rather than in the strict context of the drama of the dialogue, Parens will fail to convince some that Alfarabi's "Summary" accurately reflects Plato's genuine thinking. But even if readers are willing to grant that neither Plato nor Alfarabi is a metaphysical dogmatist, they may well ask on the basis of Parens's presentation whether they are not instead psychological dogmatists. For if we cannot trust natural philosophy or cosmology or metaphysics to ground political philosophy, why should we trust psychology to do so? How are we to know that dialectical reason can illuminate the mysterious recesses of the psyche? Especially in light of the theme of the *Laws*, one wonders how Plato or Alfarabi can be confident that philosophy is qualified to interpret or guide divine law. How do they know that the gods do not insist that we follow the letter of divine law as it is given, without the assistance or intercession of philosophy? According to Parens, the *Laws* argues that philosophy is qualified to interpret divine law because believers themselves expect their laws to be "best" at meeting their needs (pp. 10, 130–3). But why are Plato and Alfarabi confident that our needs or our understanding of those needs can be a reliable foundation for interpreting or altering divine law?

In light of Parens's claim that Plato grounds his teaching on his knowledge of politics and of the psyche, Stanley Rosen's *Plato's* Statesman: *The Web of Politics* raises important questions about the foundations of the Platonic philosopher's knowledge of political or human things. A commentary on Plato's *Statesman,* Rosen's book argues that the dialogue's notoriously dry and meticulous analysis of such subjects as the art of weaving conceals a lively examination of our capacity to understand and guide politics. According to Rosen, the philosophic Stranger leads his young interlocutor through a series of ponderous and puzzling arguments in order to lay before us the impossibility of discovering a science or art of politics. The Stranger compares the art of the statesman to the art of weaving in order to emphasize the artificiality of politics. The thrust of Rosen's interpretation appears to be that political things take on their specific character through *phronesis*, through the judgment or will or "intentionality" of particular statesmen. According to Rosen, we require the imposition of such judgments by *phronesis* because human nature is essentially "incomplete." Our nature must be shaped by the statesman's judgments about what is good and just and noble in order to protect us against a hostile nature and ever-changing cosmos. Because political things are fundamentally artificial, particular, and shifting, they cannot be comprehended either by philosophic apprehension of the "forms" or by any other models or techniques of measurement. Rosen indicates that the Stranger's endorsement of the rule of prudence (*phronesis*) and critique of the rule of law (*nomos*) rest on the belief that human beings require judgments about what is "fitting," which ultimately resist rational analysis. He argues that *nomos* is defective because it is always subject to *techne,* which is morally neutral and incapable of making judgments about virtue. But because *phronesis* cannot rule human beings without force, it must rely on *nomos* and rule in the form of *nomos*.

While Rosen's commentary on the *Statesman* is considerably more direct than earlier ones, such as Seth Benardette's *The Being of the Beautiful* (1984) and Jacob Klein's *Plato's Trilogy* (1977), it does not make many concessions to the reader. Characteristically, Rosen employs dozens of transliterations without defining them very clearly and without taking the reader carefully through the Stranger's different uses of the words. And despite his meticulous and always clever examination of the details of the text, he often makes connections and draws conclusions so quickly that one is bound to wonder from time to time whether they are Rosen's rather than Plato's. One is uncertain how Rosen can know which of the Stranger's attempts can be safely ascribed to "playfulness" or irony and which are meant seriously. One remedy to this would be for Rosen to remain more at the surface of the dialogue and to explore how the Stranger's interlocutor, the less-elusive Young Socrates, seems to understand and respond to the Stranger's arguments. But Rosen does not place much weight on the dramatic context of this admittedly undramatic Platonic drama. Despite these difficulties, Rosen develops a cogent account of the *Statesman*'s teaching about the art of statesmanship. What is less clear, however, is where Rosen's account of human nature and of the limits of knowledge leaves what he calls the "transpolitical existence of phronesis" or "philosophy" (p. 190). Rosen's persuasive account of the many obstacles to human wisdom raises, but never directly confronts, questions about the possibility and rectitude of the philosophic life. For if Plato discovers essential limits to knowledge, how does he confirm that human nature is even partially fixed or complete? Especially if Plato discerns grave obstacles to knowing what is noble and what is virtue (pp. 135, 145, 152, 173, 183–4), it is difficult to understand why he is confident that the philosophic life is best. Rosen does not, for instance, explain why Plato believes that it is wise to accept the Stranger's critique of law, especially as it applies to divine law. According to Rosen, the Stranger in the *Statesman* practices one of two kinds of Platonic philosophy. In the course of the dialogue, he is compelled to transform his "technical" manner of philosophizing into "Socratic" philosophy, which is variously described as "erotic," as a love of the transcendent forms, as proselytizing, as curing souls, and as "matchmaking." What Rosen seems to deny is that Socrates' refutational conversations themselves confirm the possibility and worthiness of the philosophic life (consider p. 138).

Peter Ahrensdorf's *The Death of Socrates and the Life of Philosophy* demonstrates how the most powerful defense of the philosophic enterprise emerges from sharing in and thinking through the political and religious concerns of Socrates' nonphilosophic interlocutors. *The Death of Socrates and the Life of Philosophy* is a careful and original commentary on Plato's *Phaedo*. Unlike other scholars who regard that dialogue chiefly as a tribute to Socrates' heroism in death and as a beautiful (if not especially persuasive) account of the soul's immortality, Ahrensdorf discerns that the dialogue's deeper purpose is to vindicate the philosophic life against powerful challenges to its justice, wisdom, and piety. According to Ahrensdorf, the principal purpose behind Socrates' notoriously dubious proofs of the soul's immortality is to help his young interlocutors see the difficulties that the philosopher, like any human being, faces in claiming to deserve immortality. In the end, Socrates means to lead them to reflect for themselves on the depth of their hope to merit happiness in an afterlife as well as on the plausibility of realizing that hope. According to Ahrensdorf, Socrates believes that it is precisely our fear of death and hope for immortality that animates in many of us a profound distrust of reason that Socrates calls "misology." If, however, we can confront our fear and, to the extent possible, overcome our grief at having to die, we may be able to perceive, admire, and enjoy the admittedly limited knowledge that is available to us in this life.

Ahrensdorf's rich account of the historic influence of the dialogue is unsurpassed. What is most impressive, however, is his extraordinary sensitivity to the drama and argument of the dialogue. He captures and amplifies the doubts and concerns that animate each of Socrates' principal interlocutors and persuasively shows how Socrates' arguments address each of them. Despite these strengths, Ahrensdorf's account of the *Phaedo* is not exhaustive. Other commentaries, such as Paul Stern's *Socratic Rationalism and Political Philosophy* (1993), focus more directly on the "metaphysical"—as opposed to the "moral" and "religious"—dimensions of the dialogue. But Ahrensdorf's approach is most compelling because it is most dialogical. Ahrensdorf pays considerably more attention to the surface of the *Phaedo* than have earlier commentators, such as Ronna Burger's *The Phaedo: A Platonic Labyrinth* (1984) and Kenneth Dorter's *Phaedo: An Interpretation* (1982). Despite Socrates' explicit claims that he shares his interlocutors' straightforward moral and pious concerns, these commentators readily claim to know which remarks Socrates means seriously and which he intends merely as consolation for his naive companions. But in following this procedure, Burger and Dorter disregard the only nonarbitrary starting point for interpreting the dialogue. Unlike Ahrensdorf, they never fully articulate the powerful questions and problems that readers need to consider in order to grasp what is at stake in the dialogue and thus never ascend through Socrates' arguments to what he seriously means to convey.

Even readers who are not persuaded by Ahrensdorf's impressively careful yet bold interpretation of the *Phaedo* are bound to learn from it. *The Death of Socrates and the Life of Philosophy* is the best reading of the *Phaedo* and one of the best examinations of Platonic political philosophy in decades.

Together these three books reflect the extraordinary richness of Platonic political philosophy. In particular, they demonstrate why the commentary is indispensable for recovering Plato's genuine thinking. Working diligently through the drama of the dialogues, they show how Plato's spokesmen sometimes speak with an eye to engaging and exposing the thoughts of their interlocutors and thus they challenge the facile assumption that Plato simply addresses us as a metaphysical dogmatist or ideologue.

**The Permanence of the Political: A Democratic Critique of the Radical Impulse to Transcend Politics.** By Joseph M. Schwartz. Princeton: Princeton University Press, 1995. 336p. $39.50 cloth, $31.50 paper.

Benjamin Barber, *Rutgers University*

Radicals have never had an easy time with politics. Enraged by injustice, drawn to Utopia, seduced by perfection, they too often see in the compromises of politics the art of the sellout to establish power. Their yearning for improvement can become the yearning for total revolution (see Bernard Yack), and the impulse to total emancipation can become the impulse to totalitarianism (see J. L. Talmon). Democratic radicalism would seem to require both solidarity ("fraternity") and equality, but solidarity may crush diversity and equality has always stood in tension with liberty. Although they embrace democracy as equality's ideal political regime, radicals thus often end up contemning democratic politics. And while pluralism may be a desirable end state, its daunting variety and invitation to conflict may stand in the way of the pursuit of justice.

In a provocative new book, Joseph M. Schwartz offers a democratic radical critique of the radical democratic impulse to transcend the politics of conflict and to overcome pluralism. *The Permanence of the Political* confronts five canonical thinkers Schwartz wants to associate with antipolitical radicalism: Rousseau, Hegel, Marx, Lenin, and Hannah Arendt. Convinced that radicalism has been "crippled by unbridled hostility to both social pluralism and rights-based liberalism," Schwartz aspires to a socialism compatible with the "plural nature of humanity"—a version of radicalism more open to rights, more in tune with the kinds of democratic radicalism espoused by Carole Pateman, Robert Dahl, Michael Walzer, Roberto Unger, Jane Mansbridge, and this reviewer (though Schwartz is by no means satisfied with this group, either).

In revisiting the canon, Schwartz is no J. L. Talmon or Karl Popper, so anxious to expose the ideological underbelly of certain philosophical positions that he cannot get straight what their authors actually wrote. Hence, even as he dwells on their "common impulse to abolish social conflict," he essays "to recognize the unique intellectual project of each thinker." The question for the reader is whether he achieves this equanimity of vision. For while he is wedded to careful analysis, Schwartz wants finally to demonstrate that the "antipolitical tone of Rousseau's harmonius regime" has an elective affinity with Hegel's "administering away of politics," which in turn is echoed in Marx's and Lenin's scientific socialism in which administration replaces politics. This "persistent flight from politics to adminstration" is finally discernible in Hannah Arendt's project as well: for though it celebrates politics, it nonetheless cedes the "social question" to experts; and on the way to elevating a certain aristocratic form of political deliberation, it not only "embraces the radical tradition's desire to transcend social pluralism," but also empties politics of its contents. As George Kateb has observed, Arendt's politics is so loftily heroic, so innocent of the social, that it deliberates only on constitutions and wars. This is not exactly the politics of the flat tax or welfare reform.

Readers will have to make their own judgments about the success of the analysis as it applies to the canonical figures. Alan Gilbert, who has written persuasively on both Marx's politics and his ethics, would probably want to contest the portrait of Marx as transcending both, while anyone willing to take the radical and pluralistic character of dialectic in Hegel seriously may conclude that Schwartz has read too much of Kojeve on Hegel and too little of Marcuse.

For my part, I am ambivalent about his reading of Rousseau and Arendt. Schwartz is certainly persuasive in arguing that Rousseau eschews political deliberation because he fears it will encourage social conflict and that he risks ending up with an Assembly that is little more than a "ritual gathering." Moreover, Rousseau does specify conditions for democracy in which conflict and pluralism are minimized so that general willing can succeed. Yet, he also understands that politics is for men, not angels. Real interest conflict turns out to be the device by which particular wills "cancel one another out" and yield a residual commonality that is the general will. He insists on taking men as they are (particularistic, conflicted) and only laws as they ought to be. While Schwartz avers he is not taking J. L. Talmon's ideological road, he sometimes seems to be consulting the "road-to-serfdom" literature's road atlas. The liberal Rousseau, the Swiss Rousseau, the prudent Rousseau are little visible in his portrait. Yet, any one of them would be far more amenable to the pluralistic politics favored by Schwartz.

Similarly, while Schwartz scores points with his skepticism about Hannah Arendt's usefulness as a model for radical democrats wishing to honor politics, he misses a core feature of her argument. He succeeds in demonstrating that the fear of diversity that Arlene Saxonhouse explored in her book about the birth of ancient political science inheres also in the ancient republican politics revered by Arendt. Yet, in indicting Arendt he accuses her not only of resurrecting ancient republican politics but also of bracketing the social question and of disavowing instrumentalism. The trouble is that she does these things precisely in the name of reestablishing politics in the face of the positivist, historicist, and Marxist assault on it. Her project would seem to be exactly in the spirit of Schwartz's: a politics innoculated against economic reductionism and historicist administration. Does Schwartz think politics is a synonym for instrumentalism? Arendt brackets the social question precisely in order to secure the autonomy of politics from the determinism of the socioeconomic—from reductionism of the kind Marx represented. What else can taking politics seriously mean than recognizing its sovereignty over economics and its autonomy from history?

While Schwartz's canonical readings can be doubted one by one, they nonetheless have a collective force. Radicals *have* disdained rights as a sanctuary for individualism, condemned pluralism as a camouflage for hegemony, and distrusted politics as the legitimation of powerful special interests. Must they? Are contemporary democrats more tolerant of the political? Schwartz thinks they have come part of the way but believes they still risk being seduced by the transcendentalist temptation. It is not clear to me, however, either that Schwartz himself is free from that temptation or that a perfect balance between radical progressivism and democratic politics is really possible.

Schwartz inadvertently evinces this difficulty when in an aside he complains that "in the name of self-defeating 'pragmatism,' the Clinton Adminstration compromises with powerful interests . . . " (p. 223). But pragmatism *is* politics, and compromising with powerful interests that are powerful precisely in that they both mirror and condition popular opinion is what politics is about. Schwartz's impatience with Clinton manifests the very radical impatience that he elsewhere assails. And who is to blame him?

Radicals presumably get fed up with politics precisely because politics often reflects power rather than modifying it. The majority is sluggish to make radical changes—revolution being the last, not the first resort of most peoples under most circumstances. The radicalism of the American Revolution was forced on it by English stupidity and intransigence. The forces that put liberty and equality in tension, that render solidarity an enemy of individualism, that make a fear of change an ally of established power are responsible for the fundamental ideological cleavage between conservatives and progressives. A politics that acknowledges the need for compromise and the ineluctability of difference may be inadequate to the tasks of wholesale reformation. Pragmatism is a virtue of politics and a vice of radicalism. *Radical democracy* may be oxymoronic, which is why politics has for radicals perhaps always been a surrogate for revolution, to be abandoned when it cannot serve radical change. Pragmatic politics is always at least a little bit corrupt—often very corrupt.

Radicals have thus found themselves poised uneasily between the real and the ideal, between logrolling and principle, between politics and revolution. Schwartz's attempt to remind them of what politics requires of them, even if it tempers the urge to revolution, is welcome. But knowing how to honor politics without yielding to the status quo, to give up on revolution without giving up on revolutionary aspirations, is a difficult task. And while I will continue to pursue a democratic politics, I am willing to forgive some radicals for deciding to leave the politics of prudence to others and for making themselves, like Rousseau and Marx, into champions of the impossible.

**Culture/Flesh: Explorations of Postcivilized Modernity.** By Michael A. Weinstein. Lanham, MD: Rowman and Littlefield, 1995. 144p. $48.50 cloth, $19.95 paper.

Nancy S. Love, *Pennsylvania State University*

Michael Weinstein's *Culture/Flesh* is an extended meditation on the human condition in postcivilized modernity. Weinstein portrays the modern project as a series of technological advances that inevitably fail to compensate fully for human finitude. Although postmoderns deconstruct modernity, they ultimately offer nothing new, only the substitution of signifiers and simulacra for selfhood. (Post)modernism, at best, affirms the completely encultured body/psyche. There is no exit from here, no road to a new transcendence: God is dead and so, as the graffiti says, is Nietzsche. Nostalgic traditionalists and/or new polytheists offer lifestyles which may decrease anxiety levels and increase self-esteem, but that is all. However, the dual legacy of human civilization—to affirm life as good and people as individuals—suggests another alternative to Weinstein: the civil savage.

The civil savage is an existential skeptic who accepts his place in the mystery of life and declares himself a part of its creative truth. His motto—"everything is possible, nothing is necessary"—leads beyond nihilism to a life-strategy Weinstein calls "erotic hedonism." It involves a commitment "to direct self-consciously one's connection to the other-than-self" (p. 54). Unlike selfish (or mere) hedonism, its "ironical devotion is a form of living-into the world, an attitude or disposition of active appreciation" (p. 68). The civil savage embraces the tension between infinite hopes and finite bodies, and thereby bridges the unavoidable distance between internal and external experience. Politically, the civil savage is a liberal, but not the "fascistic" liberal who makes choices without options, a creature who craves security above all else. Instead, his "civility is the public expression of sanity that blends vivid emotion, a taste for worldliness, and a keen judgment of finite possibility in which perception challenges judgment and judgment steadies perception" (p. 100).

Weinstein's meditation offers an important alternative to the modern/postmodern debate. It also contributes to current attempts to reconstruct the subject, especially of politics. Although Weinstein draws on the Nietzschean and the Foucauldian concepts of the overman and care of the self, respectively, the civil savage is more existentialist and psychoanalytic in character. Indeed, Weinstein regards Freud's dualism of Eros and Ananke as the best guide for an individuality "constitutively unreconciled to the world" (p. 64). As a philosophy of conduct, *Culture/Flesh* challenges the arguments of such diverse works as Charles Taylor's *Sources of the Self* (1989), Seyla Benhabib's *Situating the Self* (1992), and Judith Butler's *Bodies That Matter* (1993).

These comparisons also suggest where Weinstein's argument may need further development. The civil savage lives in a (post)modern "culture jungle." Weinstein's jungle metaphor, which alludes to a more primitive past, is revealing. Culture, for Weinstein, is far removed from the nature it fails completely to conquer. By calling that natural residuum "body" or "flesh," he dissociates it from the "nature" of primitive societies. Indeed, Weinstein treats the separation of mankind from nature as a form of progress. He also assumes that physical survival is not a primary concern in modern societies. However, indigenous peoples, even those who embrace modern technology, might challenge these assumptions. Culture may progress, but nature most likely endures. So far, nature has included the all-too-mortal bodies/flesh of human beings. Weinstein admits that "there is something that is not final about the zero point. It is impossible, perhaps, really to be above life" (p. 43). Survival with—not on—the earth may now require human beings to balance cultural advances with natural processes. If so, we moderns still have much to learn from so-called primitive cultures.

This brings me to a second and related concern. Weinstein says, "My viewpoint is masculine and the savage is male." Yet, he claims "no distinction between women and men as discrete centers of finite conscious life" (pp. viii–ix). Together these comments might suggest that Weinstein (like many others) conflates masculine with human. That is not my concern here. Weinstein claims that he writes "as a man" out of respect for difference and, more important, gender distinctions are arguably also cultural constructs. I did want Weinstein to discuss the ways culture "lacerates" and "pierces" the flesh of women, as well as men (p. 101). The absence of women in his extended meditation may parallel the denial of natural processes. I would not reassert the association of woman/nature as (M)other. But I would have Weinstein consider the (stereo)typically female experience of finitude, bearing a child. He writes, "We fast approach the time when we spring not from the earth, but from a test tube" (p. 89). Here too his metaphor is revealing. The future may bring babies from test tubes, but to date humans have been "of women born." Acknowledging this fact might cause Weinstein to qualify his portrait not only of natural as primitive, but also of others as passive (p. 68) and victims as weak (p. 102).

These are serious concerns, but they are prompted by Weinstein's valuable insights. *Culture/Flesh* is a creative, vital response to the challenges of (post)modernity. It deserves the attention of scholars committed to reaffirming the value of life.

**Liberalism without Illusions: Essays on Liberal Theory and the Political Vision of Judith N. Shklar.** Edited by Bernard Yack. Chicago: University of Chicago Press, 1996. 292p. $42.00 cloth, $16.95 paper.

Richard E. Flathman, *The Johns Hopkins University*

This is a welcome collection of sixteen essays responding to and attempting to continue the investigations and lines of reflection of one of the most eminent of recent political theorists and interpreters of the history of political thought. Following an orienting introduction by the editor, the volume divides into three parts plus an appendix that lists Professor Shklar's published works and makes available her already much appreciated 1989 Haskins Lecture in which she reflected in (for her) unusually personal terms on her life and career. Part 1 consists of six essays (by Michael Walzer, Nancy Rosenblum, John Dunn, Seyla Benhabib, Amy Gutmann, and Stanley Hoffman) that review and critique the late Professor Shklar's formulations of liberal theory. Part 2 presents five papers (by Steven Holmes, Tracy Strong, Patrick Riley, Isaac Kramnick, and Quentin Skinner) concerning French and British theorists in whose thinking Shklar had an abiding interest. Part 3, featuring essays by Dennis Thompson, Yack, Bruce Ackerman, George Kateb, and Rogers Smith, addresses issues central to liberal theory and practice as Shklar understood them. It would of course be impossible for me to respond to all of these papers; because of the great interest of Shklar's formulations of liberalism I limit my comments primarily to the essays in Part 1 of the volume. I must, however, strongly urge attention to Kateb's vigorously thought reflections concerning "The Freedom of Useless and Harmful Speech." If there is an essay in this collection that resonates powerfully with the theme of liberalism *without illusions*, Kateb's is that essay.

Deep admiration for Shklar and her work is evident throughout the volume (many of the authors studied with her and the book strongly reinforces the widely received view that she was a superlative teacher), but several of the contributors to Part 1 evidence varying degrees of discomfort with the version of liberal theory implicit in her *Legalism* (1964/1986), more evident in *Ordinary Vices* (1984), and most clearly articulated in her widely discussed essay "The Liberalism of Fear." In the latter paper, Shklar contended that "liberalism has only one overriding aim: to secure the political conditions that are necessary for the exercise of personal freedom." By far the greatest threats to freedom are cruelty and the fear that it inspires, and such fear is, as Max Weber famously argued, "overwhelmingly generated by governments. . . . The sources of social oppression are indeed numerous [but] none has the deadly effect of those who, as the agents of the modern state, have unique resources of physical might and persuasion at their disposal" (*Liberalism and the Moral Life*, p. 21, 1989). Liberalism, then, is "entirely nonutopian," and it holds that if "the agencies of government have a single primary function it is to see to it that the rights of individuals be realized." And to the extent that these political conditions are maintained, "the habits of voluntarism," exhibited first and foremost in the (variously delineated) private domain, will flourish (Ibid., p. 26). Yet more troubling to many, Shklar argued that liberalism is faithfully and permanently wedded to democracy but that this is no more than "a marriage of convenience" (Ibid., p. 37).

Apparently concerned that thoughts such as these associate Shklar with the "minimal or 'night watchman' state preferred by libertarians," Yack claims that she rejected "this version of liberalism because it rests on a completely unjustified faith that governmental power is always more threatening to our freedom and security than the power of private actors such as large corporations" (pp. 3–4). Relying heavily, as do other of the more apprehensive contributors to the volume, on Shklar's *The Faces of Injustice* (1990), Yack attributes to Shklar a much more affirmative, activist conception of the role of government and of the democratic politics that influences and, ideally, finally controls governmental activities. These views are also advanced by Benhabib, by Walzer, and especially by Gutmann, all of whom argue that Shklar's undeniably abiding concern over injustice, exclusion, and fear-creating oppression and cruelty *imply* not only a positive vision of the good for human beings (Walzer) but a social-democratic conception of politics as the chief means of pursuing that vision (Benhabib and most emphatically Gutmann). Walzer argues that fear always presupposes hope for beneficial or advantageous circumstances and outcomes. (Recall, in this connection, the beautiful essay by Isaiah Berlin—a recurrent and admired presence in Shklar's work but a troubling figure for the commentators I am now discussing—entitled "From Hope and Fear Set Free.") Similarly, Benhabib claims that Shklar's "dystopic" liberalism is incomplete, and Gutmann fills it out by arguing that it impels us not only beyond "negative" or "positive" liberalism (and freedom) but on to (social-)democratic liberalism and the welfare state.

These commentators sometimes suggest that Shklar realized and endorsed the inferences they draw from her undoubted convictions, and they sometimes write as if she mistakenly resisted conclusions to which those convictions ought to have led her. On the question of what Shklar predominantly thought, I find Rosenblum's conclusion convincing: "A number of recent commentators claim to see a shift in Shklar's later writings, away from this grim preoccupation with warding off the worst official cruelties and toward a positive theory of social democracy. It is true that Shklar was increasingly interested in democratic citizenship . . . but attempts to recruit her as a social democrat or philosopher of equality go too far" (p. 25). If I understand them, this is also the thrust of the appears by John Dunn and Stanley Hoffman. Contrary to Benhabib (p. 60), I don't detect anything notably "Prussian" in Shklar's preoccupation—underlined by both Dunn and Hoffman—with the rectitude of those in positions of political authority and power, and Dunn is certainly correct that Shklar, while convinced that an apprehensive, vigilant citizenry was indispensable, never believed that political participation is somehow elevating or ennobling. Rosenblum's nicely nuanced discussion of the "democracy of everyday life" seems to me to capture, along with Kateb's essay, what is most distinctive and distinctively valuable in Shklar's thinking.

## American Politics

**Proportional Representation and Election Reform in Ohio.** By Kathleen L. Barber. Columbus: Ohio State University Press, 1995. 383p. $45.00

V. Jerone Stephens, *Bowling Green State University*

Kathleen Barber has written a book that makes a substantial contribution to our knowledge about electoral systems and proportional representation. The book contains six chapters by Barber, including the case study of Cleveland. Four case study chapters have been contributed by political scientists and historians at Ohio universities covering variable periods between 1915 and 1960. All of the contributors address the same questions and issues concerning proportional representation (PR) and electoral reform in Ohio. The type of PR used in the five Ohio cities was the single transferable vote (STV). The cities examined are three large industrial ones—Cleveland, Cincinnati, and Toledo—and two small ones, Ashtabula and Middletown.

The individual chapters are comprehensive and are excellent contributions to the PR/STV literature on representation. The variables examined by each contributor include the political setting and the factors contributing to the adoption of PR. Race, gender, stability, and polarization in the electoral system under PR are explored in order to examine controversies and criticisms traditionally associated with PR. The analyses in the case study chapters of the book deal with the rise and fall of PR in the five cities, the results of PR for representation for various groups, and why PR was repealed. Given the increased attention to the idea of community in representation and electoral politics, including the debate over gerrymandering, these chapters are timely and important. In fact, all future research on PR that does not make use of these case studies will be incomplete.

All cities examined except Cleveland had a city manager, a small council, and at-large elections under PR; these characteristics were retained when plurality voting replaced PR. Cleveland PR/STV used multimember districts and is useful for this reason. A major issue currently being featured in the literature concerns the implementation of cumulative voting in electing U.S. House of Representative members from multimember districts. The multimember scheme for cumulative voting for North Carolina by Lew Mortimer has been discussed by Peter Applebome in the *New York Times* (April 3, 1994), and the scheme is widely reprinted elsewhere. Future research and debates can benefit greatly from the Barber research by providing us with detailed accounts about what actually happened under PR in multimember districts.

The Cleveland, Cincinnati, and Toledo cases all show the usefulness of PR/STV in electing African Americans to council. Toledo blacks elected 1 of 9 council members with 7% of the population. In Cincinnati, African Americans had as many as two of nine council members at times, an overrepresentation in council in proportion to the city's African American population (p. 189). After the repeal of PR in 1957, no more than two African Americans served on council at one time until 1992, even though the African American population grew to 38%. Cincinnati was unique in forming a Charter Party to protect the reforms. The Charter Party aided in the 1988 attempt to return PR to Cincinnati and led the 1991 attempted return. The book attributes both movements to the Charter Party (p. 328), but the 1988 movement was actually initiated by the Rainbow Coalition when Jesse Jackson noted the lack of representation of African Americans under the at-large system. The author of the Cincinnati case study concludes that PR "made only a marginal difference in council characteristics as compared to a plurality of at-large voting for a small council" (p. 207); this is not Barber's conclusion, however (p. 289). The Cincinnati author also questions whether PR would have made a difference had it been returned in 1988. The result of the 1988 election was 45.4% in favor of returning PR. The author of the Cincinnati chapter points to a lack of support from white "liberals" as a reason for the 1988 defeat (p. 207). Yet, the situation was more complex. Some whites in the Rainbow Coalition were in favor of districts, and a large number of influential white women were afraid that PR would result in the election of an anti-abortionist. Both the Democrats and Republicans vigorously opposed PR, as did the business establishment and the *Cincinnati Enquirer*. African American politicians in the Democratic Party also opposed the return of PR.

Barber has written chapters on the "Roots of PR" and "Progressives and PR," both major contributions, which provide a comprehensive account of the history of PR and who the Progressives promoting PR were. The latter chapter includes an insightful discussion of the shift of PR advocates from a concern for elite protection to a means for representing excluded groups. In a chapter on "Commonalities and Contrasts," Barber concludes that the "characteristics of council members did change in several significant respects" (p. 292), although women did not gain greatly and blue-collar representation declined. The major beneficiaries were African Americans, religious groups, and ethnic minorities (p. 289). This chapter is a good analysis of what was accomplished by PR.

The author also draws upon the five case studies to discuss the future of PR in relation to the recent court cases and controversies over gerrymandering. The chapter is well crafted and can be read with interest by specialists on PR or semiproportional systems or by the nonspecialist who needs a good summary of the literature.

My major criticism is that the book does not include a chapter directly relating PR to the literature on pluralism. In the criticisms of Guinier's work, politicians used Calhoun's work on the concurrent majority to discredit her. Calhoun's solution is, in theory, a solution of representation for pluralist electoral systems, a problem growing out of Madison's formulation of pluralism. Madison's problem was the assumption of the equality of power among groups. John Low-Beer ("The Constitutional Imperative of Proportional Representation, *Yale Law Journal*, 1984, pp. 163–88) has made the connection between group theory and PR or semiproportional solutions for group theory and systems, but he did not relate his research to the literature of political science. The inclusion of a chapter building on Low-Beer would have made an excellent book even better. We can, however, use this book as a building block to relate PR and semiproportional representation to the political science literature on pluralism, a literature that is in severe need of data on actual representative systems other than winner-take-all.

**Turning Right in the Sixties: The Conservative Capture of the GOP.** By Mary C. Brennan. Chapel Hill: University of North Carolina Press, 1995. 210p. $29.95.

William F. Connelly, Jr., *Washington and Lee University*

Mention of the 1960s typically evokes thoughts of JFK and LBJ, the Great Society and the counterculture, the Civil Rights movement and antiwar protests. Mary C. Brennan chooses instead to tell the less frequently told story of that orphan of the sixties, the conservative movement. Brennan provides an interesting and very readable account of the minuet between the conservative movement and the Republican Party during the tumultuous sixties. At times she seems to be reflecting upon the 1960s in light of the 1980s, though perhaps this is appropriate given her conclusion that "if there had been no Barry Goldwater, there could have been no Ronald Reagan" (p. 141).

Brennan's thesis can best be framed by two key quotations she borrows from conservative thinkers M. Stanton Evans and F. Clifton White. Evans concluded about the 1964 Goldwater defeat that the "relatively young conservative movement in America flexed its muscles, achieved things the experts and oracles said could never happen, and moved part-way down the road to political maturity" (p. 102). Many years later White observed that "conservatives won control of the party in 1964 but it took them fifteen years to find out what the hell to do with it" (p. 111). Evolving conservative maturity, defined in part as greater moderation and political pragmatism, is Brennan's central theme. She credits conservatives with having "exorcised the extremist demon from the movement without surrendering their principles" (p. 119).

The book progresses chronologically, beginning with the 1950s and extending very briefly to the 1980s, though the predominant focus is, of course, the 1960s. While the first half of the book (chaps. 1–3) suffers from the defect of occasional sweeping generalizations, the second half provides a fair amount of useful historical detail and background material; the reader will benefit from Brennan's solid, yeoman-like archival work. The book is marred, however, by the constant repetition of the opprobrious term "right-wingers" and the overuse of the label "lunatic fringe." (Is Dallas really "an avowedly right-wing city" [p. 50]?) The description "thoughtful right-winger" (p. 116) sounds oxymoronic, at least relative to the potential alternative "thoughtful conservative." The book is at times peculiarly journalistic in its broadbrush conclusions about particular individuals left, right, and center, including Johnson, Goldwater, and Nixon (e.g., pp. 34, 89, 90). But the most serious shortcoming of this study may be its limited exploration of conservative ideas.

Brennan is sometimes superficial and condescending in her handling of the intellectual origins of conservative factionalism; the book lacks subtlety and nuance in examining key philosophical differences within conservatism (e.g., pp. 9–10). The study would have benefited enormously from a more serious exploration of the intellectual history and ideational roots of the conservative movement. While Brennan documents fairly well the political transformation of conservatism, readers should not look here for a careful examination of the ideas underlying that transformation. Better explorations of the ideas, both left and right, underlying the politics of the sixties can be found, for example, in Samuel P. Huntington's *American Politics: The Promise of Disharmony* (1981) and E. J. Dionne's *Why Americans Hate Politics* (1991). Somewhat ironically perhaps in light of book titles, a more careful consideration of the relationship between conservatism and the Republican Party can be found in Nicol C. Rae's *The Decline and Fall of Liberal Republicans from 1952 to the Present (1989).*

Nevertheless, Brennan does provide an interesting description of the factionalism within conservatism and the crosscutting cleavages defining this political movement. To her credit, she highlights the role of money, message, messengers, and the millions of grassroots individuals central to building the conservative movement in recent decades. Her book raises important questions about American conservatism and its relationship with the GOP: Why is conservatism marked by perpetual internecine warfare? Why do conservatives often seem more loyal to principle than party and more comfortable in opposition than when governing? Answers to these questions demand careful consideration of the individuals, interests, ideas, and institutions responsible for the nature of American political parties in our separation of powers system.

*Turning Right in the Sixties* concludes by underscoring certain "valuable lessons" conservatives learned while wandering in the wilderness in the years between Goldwater and Reagan. Presumably these lessons may be useful to any aspiring movement in American politics. Brennan notes that conservatives discovered the importance of grassroots politics and learned to work together despite their differences (pp. 139–40). However, the most valuable lesson learned, according to the author, was that "dedication to principle was one thing; fanaticism was another" (p. 140). Brennan concludes that conservatives' "greatest achievement of the 1960s—the transformation of conservatism into a legitimate, mainstream political alternative" required "shedding the image of extremism" and purging it of the "taint of extremism" (p. 140).

*Turning Right in the Sixties* offers a useful overview of an important development in recent American political history. Scholars will find Brennan's carefully detailed endnotes to be helpful roadmaps to a plethora of archival resources on recent American conservatism.

**The Growth of American Government: Governance from the Cleveland Era to the Present.** By Ballard C. Campbell. Bloomington: Indiana University Press, 1995. 289p. $35.00 cloth, $14.95 paper.

Charles T. Goodsell, *Virginia Polytechnic Institute and State University*

The author is an historian at Northeastern University. He comes to a topic that has long interested political science—the growth of government—from the vantage point of the historian. While we political scientists are often restless in the presence of the historian's interest in the concrete rather than the abstract, historians can teach us valuable lessons. One lesson is simply to retain a strong longitudinal perspective as we examine the present. Social scientists are prone to analyze the interplay of currently operative variables as if there were no past; historians insist that the most profoundly important independent variable is that past.

Professor Campbell looks to American history for an understanding of why American government is what it is today. He writes with what he calls a "panoramic orientation to history" (p. 2). This is a sweeping view of eras or polity types, of which he identifies three with certainty and a fourth tentatively. These are the Republican Polity, from the Revolution to the 1870s; the Transitional Polity, from the 1880s to the 1920s; the Claimant Polity, extending from the 1930s to the 1970s; and the Restrained Polity, tentatively with us from Reagan to the present.

Elaboration of these polities forms the heart of the book, with principal attention being given to the Transitional and Claimant periods. Campbell's thesis is that, whereas until the time of the Cleveland administration fear of government permeated the American Republic, following this time government was seen as a tool of public policy and eventually as a means by which citizens and groups could satisfy claims for benefits and seek reduction of risk against an uncertain world. Accompanying this evolution were an expansion of government at all levels, a shift of power from states and localities to the federal government, and an emergence of the presidency as the focus for intervention and administration. Americans never lost their ideal of individualism and limited government, however, occasioning a possible reactive movement toward restraint in more recent decades.

This kind of analysis will not strike political scientists as new, but rather as the outline of a lecture they give every semester in one course or another. However, Campbell paints his canvas with far more detail and adroitness than we customarily do. He also insists on including in the picture developments in state and local government and not just in Washington, D.C. Another element of the story that we often ignore is World War II; he sees it, in addition to the domestic New Deal and postwar reconstruction, as one of the dramatic moments of centralized control of society through public direction of the economy, fiscal expansion, presidential leadership, and societal transformation.

A second lesson historians can offer political scientists is caution in identifying specific causes for political change. Campbell sees his "first order of business" as mapping out what policymakers did, with consideration of causes to follow this inventory (p. 7). His analysis of the causes of the growth of American government centers on industrialization and its consequences, emergent democratizing pressures, random events, and pressures within government itself. Although he presents a systems diagram for this process (inappropriately in my view), Campbell is eager to stress that causal factors interact with one other in an immensely complex dynamic, with each era being profoundly affected by its predecessor.

Again, political scientists will see this kind of argument as treading on familiar ground. But once more Campbell provides us with useful correctives. For example, he complains that we often overstate the importance of elections, particularly those of presidents, because of their high visibility rather than any necessary importance. In my view, political scientists should plead guilty to this charge. Likewise those holding various unicausal theoretical positions, such as the hero-president glorifiers, theorists of state formation, and public choice denouncers of self-seeking legislators and bureaucrats, should stand chastised.

Speaking of actors within government, one of Campbell's most original points is that we can see, over the three definite eras at least, a grand secular trend toward more extended officeholding. He sees this as occurring for both elected and appointed officials. The Republican Polity was marked by short terms and rotation of office, he notes, while government later became a long-term employment proposition with the development of full-time legislatures, the seniority system, a dynasty concept of House Speakers, a career civil service, and the emergence of powerful administrators who created empires to protect, such as J. Edgar Hoover and Robert Moses. This generalized perspective on underlying structural change in American government is a helpful antidote to our usual narrow analyses of legislative careers, the history of the merit system, or supposed budget maximization by bureaucrats.

The third and final lesson for political scientists rendered by historians is to strive for elegance of expression. Historians see themselves as humanists as much as social scientists and put great stock in telling their story with literary polish. We need more of this. We should teach our graduate students to write more than just research reports. We should encourage the self-conscious adoption of literary devices, such as Campbell's use in this book of Grover Cleveland's 1887 message to Congress. He introduces chapter 1 by noting how this message warns of the danger of an excess of federal revenues caused by the tariff. He then returns to Cleveland at several points in the narrative, showing, for example, that in that very same 1887 message the president also called for many governmental responses to perceived problems, such as modernization of the navy, extension of the postal service, construction of a federal prison, and enlargement of the Labor Bureau. At another point in the book, Campbell informs us that in 1887 Cleveland signed into law the first permanent federal grant-in-aid program. Campbell completes the literary unification of his manuscript with the observation that the contradiction of wanting both limited government and more active government that was manifest in Cleveland's time remains present in our polity to this day.

The book is a good one on its own terms. It carries out its stated objectives well. While Campbell's project is not startlingly original, it is carried out with flair, grace, and attention to detail. This reviewer's only real complaint is that the author might have given more attention to our current era. He discusses only the Reagan period and does so in just a few pages, leaving several interesting years and events thereafter untouched, even though they would have substantiated his perception of a late-century trend toward restraining government. But I say this as a political scientist obsessed with generalizing about the present, not an historian determined to anchor us in the concreteness of the past.

**The Clinton Presidency: First Appraisals.** Edited by Colin Campbell and Bert A. Rockman. Chatham, NJ: Chatham House, 1996. 408p. $32.00 cloth, $22.95 paper.

Norman C. Thomas, *University of Cincinnati*

Assessing presidential performance is a favorite sport of scholars, journalists, and other political junkies. When undertaken by serious students of the presidency, the task is difficult because the criteria for evaluation are imprecise and often vary widely. To evaluate a presidency at midterm is even more challenging, as there are many loose ends, much remains to be done, and the president and his administration are still learning. Colin Campbell and Bert Rockman, the intrepid editors of this volume, and their distinguished contributors are undaunted and inform us that they recognize that "half-time is still half-time" (p. 13). Their combined efforts result in a successful reprise of the editors' highly regarded midterm assessment of the Bush presidency.

The contributions include Charles O. Jones, "Campaigning to Govern: The Clinton Style"; Campbell, "Management in a Sandbox: Why the Clinton White House Failed to Cope with Gridlock"; Barbara Sinclair, "Trying to Govern Positively in a Negative Era: Clinton and the 103rd Congress"; David M. O'Brien, "Clinton's Legal Policy and the Courts: Rising Disarray or Turning Around and Around?"; Joel D. Aberbach, "The Federal Executive under Clinton"; Harold W. Stanley, "The Parties, the President, and the 1994 Midterm Elections"; Graham K. Wilson, "The Clinton Administration and Interest Groups"; George C. Edwards III, "Frustration and Folly: Bill Clinton and the Public Presidency"; Paul J. Quirk and Joseph Hinchliffe, "Domestic Policy: The Trials of a Centrist Democrat"; Larry Berman and Emily O. Goldman, "Clinton's Foreign Policy at Midterm"; Rockman, "Leadership Style and the Clinton Presidency"; and Walter Dean Burnham, "Realignment Lives: The 1994 Earthquake and Its Implications."

These pieces analyze President Bill Clinton's performance in his first two years from several vantage points: the institutional and political contexts within which he has operated; his management and leadership styles; relations with Congress, the legal system, and the federal executive; relations with the political party system, interest groups, and the public; domestic and foreign policy developments; and the 1994 congressional elections. Three recurring themes tie the contributions together: the constraints that context (the constitutional separation of powers and the contemporary political environment) impose on presidential achievement; the disparity between Clinton's policy goals and the context in which he has had to operate; and Clinton's personality and leadership style.

The contributors' observations on context suggest that it is surprising that Clinton has accomplished as much as he has. In addition to the difficulties of operating within a system of separated institutions that is not designed to produce major policy changes quickly, Clinton has had to contend with a sour public mood engendered in part by cynical media coverage of politics and policy, an election victory (43% of the popular vote) that provided little by way of a mandate for change, ideological splintering among congressional Democrats, and tensions between traditional and newer members of the Democrats' interest group coalition. Clinton's dilemma, which any Democratic president faces in the contemporary era, is that to be elected he must campaign as a moderate New Democrat but in governing he must also respond to demands from the liberal core of his party. His problem, as Rockman puts it in his essay, is that our "governmental institutions are designed to lead presidents to safe, low-yield strategies, but popular culture and the claims of direct democracy tend to direct presidents toward high-risk but potentially high-yield strategies" (p. 328). Moreover, opposition members of Congress stand to gain more electorally by obstructing a president than by compromising with him.

The gap between Clinton's policy goals and his political resources appears in several of the contributors' analyses of his handling of the gays in the military issue, the 1993 economic legislation, the failure of health care reform in 1994, and the strong emphasis on diversity in appointments. In each instance he overreached with consequences that weakened him politically: supporters were often disappointed, opponents discovered that he could easily be rolled, and the public acquired the impression that he was ineffective. The 1993 passage, with Republican support, of the North American Free Trade Agreement is an exception to this pattern, but no contributor suggests that a general bipartisan approach to policy would have been viable.

Although most contributors appear to believe that structural and other contextual factors are more important than personal style and leadership skill in explaining presidential success, they all attach considerable significance to Clinton's persona. The collective portrait that emerges is not flattering. The contributors see Clinton as intellectually gifted and remarkably energetic, but undisciplined; a masterful communicator, with skills that rival those of Ronald Reagan, but whose inconstancy with respect to his goals and policies leaves him unable to define himself or his administration effectively for the public or Congress; and a career political executive inattentive to the importance of organization and staffing to presidential effectiveness. There is thus both admiration and perplexity in their views of Clinton.

Space constraints preclude a discussion of the individual contributions, all of which have much to commend them. Two deserve special mention. Burnham provides an empirical and historical analysis of the momentous 1994 midterm elections that will be of lasting importance to all students of American politics. He suggests that it has the "characteristics of an old-style partisan critical alignment ... from which bearings will have to be taken for a long time to come" (p. 363). Berman and Goldman's analysis of Clinton's foreign policy is an outstanding discussion of the problems of international leadership in the uncertainty of the post–Cold War era. They question whether Clinton's "domestic national purpose" orientation can provide either coherence or focus to policy (pp. 297–8).

This book leaves one with a deep sense of pessimism about the prospects for successful presidential leadership, at least from a president whose goals require a positive role for the state and a competent federal government. Contextual constraints and limited latitude for policy change imposed by the large structural budget deficit afford little basis for optimism. Perhaps if the contributors had been able to observe Clinton's performance in his third year, under much more adverse conditions than he had to face in the first half of his term, they would have been slightly more upbeat. He has managed to define himself and his goals more clearly; turned foreign policy to his advantage in trouble spots such as Bosnia, the Middle East, and Northern Ireland; and established more orderly processes in the White House. His standing in the polls remains stable, with somewhat more Americans approving than disapproving his performance. And, at least so far, he has outmaneuvered the leaders of the Republican opposition in the battle over their Contract with America. Perhaps what the Clinton presidency tells us is that for the time being divided government and substantial policy gridlock are the norm. Polls and election results since 1968 suggest that the public is not comfortable with either political party being fully in charge of the government. Although this book provides few answers about the current state of the presidency and American politics, its questions are serious and thought provoking. It should not be missed.

**Experts and Politicians: Reform Challenges to Machine Politics in New York, Cleveland, and Chicago.** By Kenneth Finegold. Princeton: Princeton University Press, 1995. 264p. $39.50.

Clarence N. Stone, *University of Maryland, College Park*

The study of urban political development has come a long way since the days when city politics was treated as a clash between a machine orientation based in immigrant communities and a reform orientation supported by an old-stock population. As sophisticated historical research has refined our understanding, social determinism has given way to analyses in which political outcomes are contingent on strategic choices made in complex situations. Focusing on the Progressive era, Kenneth Finegold's examination of reform politics in New York, Cleveland, and Chicago is a worthy addition to the historically informed study of urban political practice.

Finegold's book is a model of comparative case analysis. Taking three cities similar in size, social diversity, and economic base, the author identifies factors that can account for variations in policy and politics, within and among the three cities. The Progressive era was a period of much intellectual ferment about policy innovation and the functions of government, and this ferment spread reform ideas widely across the nation's large cities. Finegold's analysis takes seriously the role of ideas but asks how, through the process of reform, they came to be acted on in different ways in different circumstances.

Where many previous studies have examined the complexities of machine politics, Finegold focuses on reform. He shows that traditional reform centered on ideas of neutral competence and efficiency was only one variant. A second variant, municipal populism, had a different appeal. It favored wide public provision of services to the citizenry as well as government ownership of key utilities (the latter, in order to provide cheaper rates to consumers). Progressive reform combined the two variants and appealed to a broad and class-inclusive coalition, though it proved to be a difficult program to carry off and sustain.

In Finegold's analysis, experts served as a mediating force between ideas and electoral politics in the Progressive period. When independent of business control and brought into the orbit of elected officeholders, experts were able to formulate a range of policies and guide their implementation in such a way as to widen the appeal of reformers. Thus, the manner and degree to which experts are incorporated into the governance of a city help explain the course of electoral politics.

Finegold offers a detailed analysis of shifting patterns of electoral support in the three cities and uses this analysis to demolish the notion that social group predicts political choice. Instead of group affiliation determining political preference, voters proved to be purposive creatures who assessed and responded to alternative programs of governmental action. While not denying that party regulars made effective use of their social attachments and their control of material benefits, Finegold shows that policy proposals and records of performance in office also carried great weight with urban electorates. Officials knew this and were attentive to programmatic concerns. Indeed, the line between machine and reform politics turns out to be a great deal less distinct in practice than in concept. Successful politicians were often adept at what Finegold calls "the politics of balance," that is, at "maintaining [an] organizational base while doing just enough to attract the reform support needed to stay in power" (p. 168).

When supported by the capacity to frame a realistic program and carry it out, the progressive reform experience suggests that politicians can move beyond the provision of selective incentives and make principled appeals. Strange as it may sound in a period so cynical as the present one, Cleveland's Newton Baker built a program around "civitism" (love of one's city) and gained political support by such measures as appropriating money for a municipal orchestra. Of course, concerns with collective goods are not totally missing from the city scene today, but they are more likely to take the form of a new stadium than money for a symphony orchestra and more likely to be defended as an economic-development measure than as a matter of civic pride.

Overall, what Finegold does he does very well—explain the complex character of reform, illuminate the role of the expert, show how political leadership and strategy can shape coalitions, and link shifting patterns of voting to the policy positions and performance of officeholders. *Experts and Politicians* is a major accomplishment and a valuable contribution to urban political inquiry.

As with most worthwhile studies, the book, however, leaves some significant questions unanswered. The main one is why reform is so hard to sustain. For example, Finegold reaches the conclusion that "it is easier to create coalitions while in opposition than to consolidate them in power" (p. 167). But the book does not explain exactly why that is the case. Do experts have a role in sustaining as well as bringing together a reform coalition? If so, are there barriers to that role? Once a wave of new ideas is introduced, does the expertise of professionals tend to move them out of contact with the concerns of elected officials or the general citizenry?

The issue of reform sustainability can be approached through particular questions and comparisons as well. After gaining power in Cleveland, why did reform subsequently lose so much ground? Progressive reform made less initial impact in New York, but it made significant reappearances under La Guardia and Lindsay. Yet, these progressive administrations subsequently also proved unsustainable. Why? These questions are largely beyond the area explored.

Finegold makes effective use of comparisons to explain variations in reform, but the overall pattern he describes is one of its transiency. Given the difficulty of sustaining reform, might one conclude that Plunkitt of Tammany Hall was not terribly far off, after all, in saying that reformers are "mornin' glories"? Finegold does not fully join that issue.

A book seldom resolves all of the questions it raises, but one can hope that Kenneth Finegold will continue to address the topic of urban reform and in the process tell us more about why reform coalitions are so hard to sustain. He is a skillful analyst and clearly has much to offer, as *Experts and Politicians* makes plain.

**Regulation in the Reagan-Bush Era: The Eruption of Presidential Influence.** By Barry D. Friedman. Pittsburgh: University of Pittsburgh Press, 1995. 261p. $59.95.

Richard W. Waterman, *University of New Mexico*

Barry Friedman's new book is a solid contribution to the literature on presidential influence of the bureaucracy. It is one of the few books, along with the excellent prior work of Edward Paul Fuchs (*Presidents, Management, and Regulation,* 1988), to examine in considerable detail the presidential role in the review of prospective agency regulations. While the Friedman book refers to regulation in the Reagan-Bush era, for the most part it is really a book about the Reagan presidency's regulatory revolution. Through interviews with agency and other relevant personnel, Friedman demonstrates how officials from Reagan's Office of Information and Regulatory Affairs (OIRA) employed cost-benefit analysis in an attempt to target wasteful and inefficient government regulations. In this process, the Reagan administration also substantially increased the president's influence over the regulatory bureaucracy. Often times it did so in a heavy-handed manner that denied groups supportive of social regulation a voice in this review process. Partly as a result, much of this gain in presidential influence turned out to be transitory.

In describing how Reagan administration officials extended the presidential role in the rule-making process, Friedman raises an interesting question. It is clear why officials from the pro-business, antiregulatory, economically conservative Reagan administration wanted to use cost-benefit analysis to constrain the development of new government regulations, but why did Congress, the courts, some interest groups, and, in many cases, the regulatory agencies choose to go along with this antiregulatory agenda? Friedman finds the answer in "exchange theory," which posits that a more powerful actor will "succeed in exacting compliance" from a less powerful actor, "but the more powerful actor is expected to offer his good will, his approval, or some other resources in return." In short, exchange theory posits that Congress, the courts, and the agencies went along with the Reagan administration so long as they perceived themselves as gaining something from the exchange. When their vital interests were threatened, however, they opposed the Reagan initiative vehemently and quite often successfully. Because the process often threatened vital interests, Friedman concludes that the Reagan administration ultimately was much more successful at delaying new regulations than it was at killing them outright.

The book's focus on exchange theory is interesting and clearly represents a contribution to the presidential and regulatory literature. Friedman, however, had a chance to make an even more lasting theoretical contribution to the political control of the bureaucracy literature by applying his results to the principal-agent model. While he discusses this theory briefly and belatedly, he misses one of the most interesting theoretical contributions his work could have offered. One of the key assumptions of the principal-agent model is the idea that there is an *information asymmetry* between the principals (such as the president and the Congress) and their bureaucratic agents. The asymmetry favors the agent, thus mitigating against principal attempts at political control of the bureaucracy. One solution to the information asymmetry problem is for principals to more aggressively monitor the behavior of their agents, thus reducing the asymmetry.

Friedman's entire book is about the role of information in this principal-agent relationship. He provides an excellent analysis of how the Reagan administration used cost-benefit analysis (through Executive Order #12291) and administrative central clearance (through Executive Order #12498) to (1) more effectively monitor the behavior (e.g., rule- and regulation-making activities) of bureaucratic agents and (2) attempt to reduce the information asymmetry by creating a centralized information-gathering/evaluating mechanism directly in the Executive Office of the President (EOP). Executive agencies, such as the Environmental Protection Agency and the Occupational Safety and Health Administration, were then forced through EO #12291 to submit proposed regulations to OIRA for evaluation and possible approval. About halfway through Reagan's eight years in office, EO #12498 was issued. It created a centralized mechanism for controlling even nascent ideas regarding future rule-making proceedings, with OIRA and OMB again organizationally playing the centralizing role. While Friedman provides numerous examples of agency recalcitrance and, in some cases, even agency success in maneuvering through this comprehensive oversight process, the fact is that the two executive orders provided the Reagan administration with a solid mechanism for ameliorating, to a considerable extent, the information asymmetry problem as it related to the rule-making process. This may be why the Clinton administration has not fully abandoned the Reagan approach, but rather has shown evidence of institutionalizing it.

Most of the political control of the bureaucracy literature has ignored the relationship between cost-benefit analysis, central clearance procedures, and the information asymmetry problem, I think, because they have tended to focus on the compliance monitoring and enforcement stages of the regulatory process rather than on rule-making. As a result, they have largely overlooked a critically important stage in the regulatory process that is directly relevant to principal-agent models.

I wish Friedman had more fully developed the theoretical implications of this work for principal-agent models. While he does not do so here, one can hope that he will undertake this task in a future research project. A great book on the applications of cost-benefit analysis and central clearance procedures to principal-agent models is waiting to be written. With this book Friedman provides a useful, if indirect, introduction to this debate. Let us hope he fully engages in this debate in the future.

**Saving the Constitution from the Courts.** By William Gangi. Norman: University of Oklahoma Press, 1995. 326p. $45.00.

Gary L. McDowell, *University of London*

On the eve of his controversial nomination to the United States Supreme Court, Judge Robert H. Bork wrote that there was a torrent of constitutional theorizing pouring from the law schools. Time has not served to stanch the flow; if anything, the ideological frenzy surrounding his nomination has only contributed to the problem. It becomes increasingly difficult to find any traces of the original Constitution beneath the floodwaters of academic opinion.

It is not merely scholarly sins that have eroded the Constitution, of course. The judges and justices themselves are guilty as well, albeit aided and abetted by their academic suppliers of misconceived ideas. The outrageous and textually indefensible decision of the Supreme Court in *Planned Parenthood of Southeastern Pennsylvania v. Casey* (1992) (a case oddly ignored by this author) is but one example of such erosion and bears impressive testimony to Professor Gangi's point that so-called conservatives are as guilty as liberals when it comes to twisting the Constitution to fit their preconceived notions of social justice. In *Casey* justices Souter, O'Conner, and Kennedy—Reagan and Bush appointees all—were able to out-Warren anything the Warren Court ever did; by the standards haughtily imposed by their plurality opinion, those three justices perceive themselves as Platonic guardians from whom we are obligated to take our political and moral bearings.

Thus, a book bearing this title is a most hopeful sign. Professor Gangi begins with a satisfying premise: The Constitution matters, it is more than what the judges say it is, and it is worth recovering because at the heart of that Constitution is our fundamental commitment to popular government. It is precisely our abiding dedication to self-government that is most endangered by a freewheeling judiciary bent on doing justice. Unfortunately, that promising point of departure gets lost in the shuffle. This is a book that promises far more than it delivers. And that is a shame, for the defense of the historic Constitution has never been more important than it is now.

The late Fred Rodell once famously noted that there are only two things wrong with legal writing: one is style, he said, and the other is substance. This book is an exemplar of both defects. On the one hand, it is written in a personal, touch-me, feel-me, kind of way; such familiarity can only breed contempt. One does not really need to know the author's feelings as opposed to his ideas; his personal "journey" takes us nowhere. And Professor Gangi's insistence on forcing his argument into a preconceived and unconvincing psychological paradigm about temperament and attitudes irritates rather than elightens. Wrestling James Madison to the couch is not exactly the most convincing way to take seriously his principles of constitutionalism against the onslaught of contemporary revisionism.

Another nagging style problem weakens this book's central argument: It is cluttered with the most staggering number of errors one can imagine. We learn for example, that *Dred Scott v. Sandford* was handed down in 1856, not 1857; Stanley Fish becomes Stanley Fisk; Eugene Hickok from one footnote to the next suddenly becomes William Hickok; and, most appallingly, with but few exceptions Chief Justice Rehnquist is identified as "Rhenquist."

Along these same lines, it is worth mentioning that the decision to print the text in side-by-side columns was a poor one; the up-and-down exercise is an unnecessary distraction. Moreover, the nominal effort that went into what is called an index was a complete waste of someone's time. Raoul Berger, for example, who appears throughout these pages, is given but three entries; others who are also frequently present do not appear at all in the index. The absence of both a bibliography and a case index is especially puzzling.

The devil, as they say, is in the details, and these details are worth mentioning because they point directly to what is the major substantive weakness of *Saving the Constitution from the Courts*. This is one of the most poorly edited books one is likely to read. The inattention to spelling, dates, and names that mar this book and the decision to forego various scholarly apparatuses that would make it usable obviously reflect a more thoroughgoing editorial indifference. This book is a bucket filled with trowels of information scraped from a variety of sources but lacking clear differentiation and thus clear argument. It reads more like a review of the literature than a book as such. What has been published as a book is in fact a manuscript that has not found its voice. Instead of a clear and compelling argument, the reader confronts a muffled roar. It is at this level that Professor Gangi's important argument gets lost.

The weaknesses here are not simply the fault of lazy editors, however. There is also the deeper problem that much of what Gangi presents has been argued elsewhere and more convincingly. Most of what one finds here is derivative, from writers such as Christopher Wolfe, H. Jefferson Powell, and, most significantly, from the many books and articles by Raoul Berger. Even the heart of Gangi's thesis—that the authors of *The Federalist* posited a theory of limited constitutional government that has been increasingly ignored by judges bent on doing the right thing—is hardly new.

One would like to think that *Saving the Constitution from the Courts* would add intellectual weight to the right side of the argument in the effort to defend the Constitution from both constitutional law and constitutional theory, but that is a highly doubtful proposition. The most one can hope for is that the repetition of the sound arguments of *The Federalist* against the intellectual pretensions of our age may be its own reward; in this case, it will have to be.

**Everybody's Children: Child Care as a Public Problem.** By William T. Gormley, Jr. Washington: Brookings Institution, 1995. 243p. $38.95 cloth, $16.95 paper.

Jane Waldfogel, *Columbia University*

William Gormley accomplishes three very important things in *Everybody's Children*: He makes a compelling case for government intervention in the child care market; he provides an analytic framework for thinking about that intervention; and he provides a wealth of information about the workings of that market. The result is a volume that is likely to be the definitive work of child care policy analysis for some time to come.

Even those who know the child care market well are still likely to learn something new from *Everybody's Children*, and for novices the book serves as a wonderful introduction to this policy area. Gormley presents the basics of the child care market in a clear and engaging manner, but the presentation is by no means elementary. This book has been meticulously researched and is carefully footnoted, thus providing both an accessible introduction to the topic and an entryway to further reading and research.

Gormley's goal in this book is not just to describe the current state of the child care market but also to make the case for government intervention, and this he does very effectively. He argues, first, that we as a society should care about child care because it is important for children, parents, and society and because children are unable to advocate for themselves. He next provides several reasons why government should intervene in the child care market. On efficiency grounds, quality child care would offer externalities (benefits not just for the individual child and family but for society more generally), yet, the supply of child care may be less than optimal due to market failures. On equity grounds, quality child care would be important in redressing inequality, which the private child care market left to itself might just reinforce. Finally, on pragmatic grounds, government intervention in the child care market may be required in order to help attain other government objectives, such as having children enter school ready to learn or having parents able to leave welfare for work. In choosing an analytic framework with which to weigh policy options, Gormley rejects both the work-family perspective, with its view of child care as necessary to enable the employment of women, and the parental perspective, with its focus on the "iron triangle" of quality, availability, and affordability of child care. Neither of these perspectives is adequate, he contends, when it comes to deciding among competing policy options. Instead, he proposes a societal perspective, with an emphasis on principles of justice, efficiency, and choice. This, he demonstrates, facilitates institutional policy analysis because it allows one to balance sometimes competing interests.

In describing the operations of the child care market, Gormley is particularly strong in his analysis of its two weakest sectors: the for-profit sector and the largely unregulated family day care sector. It is well known that for-profit day care centers, particularly those that are part of large chains such as KinderCare, tend to be less expensive than other centers; it is also widely believed that they offer lower-quality care. Gormley shows that this is in fact the case and documents the reasons why: All else being equal, for-profit centers have less-qualified teachers, fewer teachers, and higher teacher turnover. What can we do about it? Although we might be tempted to regulate for-profits out of existence, we can ill afford to do without them, at least in the short-run, since they constitute one-third of the American child care market. Gormley suggests that it may be more practical to work with this sector to improve quality by using a mix of self-regulation, tougher enforcement, and the threat of public exposure. Gormley is also a pragmatist when it comes to recommending policy to improve quality in family day care homes. The vast majority of these homes are presently unregulated, either because they are exempt from state regulations or because they are operating illegally. To bring family day care providers into the formal sector, he points out, we will have to make it worth their while by, for example, setting up networks that give providers access to substitute caregivers, lending libraries, and other resources that they value.

The strongest part of the book is the institutional analysis of the formal and informal child care infrastructure. Gormley has done a great deal of research on child care regulations and speaks very convincingly of the failings of the formal regulatory structure. Part of the problem is inadequate enforcement, but there is also a problem of competing interests, since regulations to improve quality may also drive up costs and/or reduce availability. He also details the many information problems that plague the child care system. For example, he points out that child care resource and referral agencies provide a wealth of information to parents about how to choose a child care provider, what providers exist in the community, and which have openings, but they do not provide the information parents really need, namely, which providers are good and which are known to be bad. Resource and referral agencies usually have this information about providers in their community, but they are not mandated to share it with parents and reasonably fear repercussions if they do. Thus, we rely to a (too) large extent on parents to be informed purchasers of child care and enforcers of quality, even though they are neither well suited nor well positioned to carry out this responsibility. Gormley also covers the wider array of institutions that are involved with child care, with one notable exception: He mentions schools only as providers for school-age children, ignoring the growing involvement of schools in providing programs for pre-school-age children.

If the book has a flaw, it is that the set of reform models outlined in the final chapter, "Reinventing Child Care," has little relationship with the analysis that precedes it. As Gormley himself emphasizes in the policy sections at the conclusion of earlier chapters, in this enterprise there are no easy answers. Armed with the information he has provided about the complexities of this market, the reader is likely to want to see his prescriptions for reinventing child care more fully developed and more closely linked to the remainder of the book.

In spite of this one shortcoming, the book succeeds as a work of policy analysis because Gormley understands this policy area so well and is able to convey his understanding. Although the policy conclusions in the final chapter do not hold up to the standard Gormley himself sets, the policy recommendations within the other chapters do. This is clearly an analyst who can speak with authority about the child care market, and it behooves us to listen.

**Candidate Images in Presidential Elections.** Edited by Kenneth L. Hacker. New York: Praeger, 1995. 201p. $18.95.

Mary E. Stuckey, *University of Mississippi*

*Candidate Images in Presidential Elections* is based on the beliefs that elections are central to the maintenance of American democracy and that political images—especially images of candidates—are central to elections. The book unites a variety of studies, all of which are concerned with questions pertaining to the content of images and how they are constructed and communicated as part of the campaign process. Such questions are important because, in Hacker's words, "candidate images mediate political messages and voter thoughts about those messages [and] perceptions of candidates are significantly related to how voters make judgments about candidates" (p. xii). The various chapters detail existing knowledge about the connections between candidate images and voter behavior, as well as positing new ideas and suggesting promising areas for future research. The chapters do assume a certain amount of knowledge on the part of the reader, making the volume more suited to a scholarly audience than for the undergraduate classroom.

The book is divided into two sections, one dealing with research and theory and the other treating research methods. Despite the problems caused by the unevenness of most edited collections, and present to a degree here, readers trained in the disciplines of political science, sociology, communication, and history will find much in this collection, ranging as it does across extensive epistemological and methodological terrain, that is both enlightening and useful.

In the first section, for instance, there are discussions of the standard models and theories of voting and campaign research, with chapters on advertising (Kaid and Chanslor), debates (Zakahi and Hacker), the rational voter model and its alternatives (Kendall and Paine), and political socialization in a campaign context (Savage). But these models and theories are not merely given yet another standard treatment. Instead, there are new ways of synthesizing the literature, such as in the Hellweg chapter; new challenges to these models, such as the one offered by Kendall and Paine; and new ways of conceptualizing often-studied material, such as Hacker's offering.

There is also much to excite scholars of campaign processes in the section devoted to methodology, as the authors bring a variety of methods to bear on material that has hitherto been the province of a very few methodological perspectives. There are chapters here on meta-analysis (Hellweg and Spitzberg), on semantic differential measurement (Kaid), on intensive analysis (Nimmo), on a Rashomonian approach (Kruse and Kendall), and on linguistic discourse analysis (Hacker).

As research has become increasingly multidisciplinary in scope, it has also tended to become more fragmented. Researchers working in similar areas, or in the same area but from different methodological perspectives, may never encounter one another's ideas. As numerous scholars have noted, this is a particular problem for scholars of political communication, whose work often parallels research in public opinion, elections, and political behavior. All of the authors included here have provided analyses that illustrate how profitable the building of bridges between disciplines can be. While most of the authors are affiliated with departments and schools of communication (Savage is the lone exception) rather than political science, they display impressive command of a wide array of literatures and are equally at home with analyses originating in political science, psychology, sociology, and communication.

For example, in "Campaign and Candidate Images in Presidential Elections," Susan Hellweg synthesizes the literature from political science, communication, and sociology, covering such topics as how images are defined and studied, the relationships between images and voting, the role of voter and candidate demographics in the evaluation process, the relationships between images and issues, how images are conceptualized by voters across elections, and the role of the media, concluding with suggestions for future research based on this synthesis. Hellweg provides more than a mere literature review here, however; like most of the other authors included in this volume, she both synthesizes and critically analyzes the material, providing a valuable service for scholars interested in the larger questions of campaign dynamics. Similarly, in their discussion of "Political Images and Voting Behavior," Kathleen Kendall and Scott Paine review the rational voter literature as well as the active voter model before positing a "cybernetic" alternative model as a "hybrid . . . able to account for both stability and change" (p. 33).

By and large, the chapters included in this volume challenge researchers to go beyond the narrow definitions of their fields of expertise and to reconceptualize both their research questions and their methods for addressing those questions. In this context, it must be noted, however, that the short shrift given some of the methodological issues (the Kaid "chapter" on semantic differentials, for instance, is all of four pages long) is puzzling.

If this volume has a consistent weakness, it is that the connections between the various chapters are not as well articulated as one would like. More dialogue among the authors, or at least a concluding chapter that draws out the points of intersection and divergence, would have been helpful. In a similar vein, not all of the chapters include a section on future directions for research. These sections are useful and interesting when they do appear; consistency in this regard would be helpful for both substantive as well as aesthetic reasons.

Nonetheless, Hacker has brought together some of the most prominent and productive scholars in the field of political communication, and he has done so in a way that provides important methodological and substantive contributions to the field. This work will be valuable for scholars from a variety of theoretical perspective and invaluable to their students.

**The Future of the Space Industry: Private Enterprise and Public Policy.** By Roger Handberg. Westport, CT: Quorom Books, 1995. 192p. $55.00.

Nathan C. Goldman, *South Texas College of Law*

*The Future of Space Industry* is twice an excellent book—once as a piece of original research and twice as an important volume in the growing subdiscipline of space policy within the subfield of science and technology policy. The book analyzes the change and direction of space industrialization or commerce in the latter third of the twentieth century with an eye toward predicting the future of space commerce in the dawning millenium.

Professor Handberg reviews the history of space industry, providing several telling conceptualizations or variables to help social scientists and lay readers understand the relationships between policymaking and the actions among the space players. Much of his argument is organized around a tripartite division of space players among (1) the public military sector, (2) the public civilian sector, and (3) the private sector. Handberg details the unique institutional attributes, mindsets, and goals of each sector, and then develops a dialectic that reveals the synergies and tensions among these sectors.

That analysis is interesting, and what gives it its overarching dynamic is the author's presentation of the shifting relationships among the sectors over time. This, he accomplishes with a three-era paradigm of the space industry. The first, the "Developmental Stages," 1946–66, is the classic Cold War era. The second, 1966–86, he calls the "Initial Separation," during which private space activities really began. The most recent era, after 1986, discusses the changes in the space industry initially wrought by the *Challenger* accident and later by the dissolution of the Soviet Union and the end of the Cold War.

During the classic period, there was no private space sector in the sense of a market-driven private economy. Indeed, government needs, almost entirely foreign policy needs, dictated the direction and details of space activities. NASA and DOD was the market, a market in which cost was hardly an obstacle; only the results—beating the Russians to the moon—really mattered.

In the second era, after we had beaten the Russians, the international imperatives were changing. Suddenly, money had become a factor; the big aerospace companies could not count on the largesse of a grateful nation. At the same time, the new technologies created a vision of new wealth from space endeavors. Many start-up companies were created, and some established companies attempted entrepreneurial efforts. Indeed, space telecommunications became a thriving enterprise in this second period. Nonetheless, the dominant force in space industry remained the government—both through its reduced, but still awesome pursestrings, and through its regulatory and operational grip on all space activities.

After the *Challenger* accident much of the space industry was totally revamped. The shuttle did not fly for almost three years; in that time, many of the start-up companies that depended on the shuttle went out of business. More important, the entire launch industry was reshuffled. The Reagan administration issued a directive that no commercial satellites could fly on the shuttle. Instantly, a private launch industry had to come into existence.

Yet, the problem for U.S. space industry comes from another characteristic of the third era: the internationalization of space activities in general and of space industry in particular. Internationalization is forcing the companies both to compete and to cooperate on a transnational scale. This process is forcing companies, long dependent on one government's largesse, to develop a worldwide marketing strategy. Space companies for the first time have the opportunity to fail big as well as to succeed big. Domestic and world events in this third era are conspiring to spawn a full market economy for space activities.

Although I disagree with some of his interpretation of the events and strategies, Professor Handberg suggests in parts of the book that the recent consolidation of many space companies is both mandated and required by the smaller government market and the greater international marketplace. Conversely, he suggests that the future in space business belongs to the niche players who can move quickly and expertly into the markets created by the fluid dynamics of modern space enterprise. These disparate tendencies can be reconciled, but the book does not make the attempt. This thorough and well-argued book has very few of this kind of problem.

Professor Handberg's book joins other efforts by social scientists to discuss the public and private policymaking millieus in areas that overlap many disciplines. Space policy is a relatively new subdiscipline. To this important issue, social scientists can bring all the methodologies and other means of analysis developed in their disciplines. The challenge of public policy in general has always been to move beyond reporting to analysis and, more important, to testable hypotheses. Professor Handberg presents a well-argued story, offers important insights into the working of the varied sectors of the space enterprise (especially the private), and brings useful data to back up his arguments about the present and future dynamics of space industry. This is an important book in a field that will only become more important in the coming years for social scientists and for the public as a whole.

**The Foundation of Merit: Public Service in American Democracy.** By Patricia Wallace Ingraham. Baltimore: Johns Hopkins University Press, 1995. 170p. $45.00 cloth, $14.95 paper.

Peri E. Arnold, *University of Notre Dame*

As political science and public administration drifted apart, knowledge of the American federal public service became the province of the latter discipline and scarce among political scientists. Yet, the public service's character and quality significantly influence government's efficacy and responsiveness, fundamental concerns of political scientists. If they lack a grasp of the American public service, political scientists are ill-prepared to assess governmental performance.

Knowledge of the American public service is not easily gained. Federal personnel systems are an accretion of arcane regulations and practices accumulated over time. Occasional reforms promise clarity but are more likely to plaster new complexities over accumulated policies. How can one make sense of the public service? Patricia Wallace Ingraham's *The Foundation of Merit* helps answer that question.

A leading scholar of the American civil service, Ingraham brings ambitious aims to this small volume. The book surveys the values and political goals that shaped the development of the federal public service, examining changing personnel policies and detailing efforts at control by political executives. Ingraham also addresses the possibilities for reform in the public service to reflect a changing work force, changing citizen expectations, and changing public policy domains.

*The Foundation of Merit* is reminiscent of an earlier fine book on the public service, Frederick C. Mosher's (1968) *Democracy and the Public Service*. Like Mosher, Ingraham details the public service's development within the American political context. More effectively than Mosher did for his era, Ingraham explains the problems of the public service in light of the contemporary governmental context.

Ingraham's animating insight is that the problems of the federal public service result not from the separation of merit from politics, but rather from the association of merit and politics in the American experience. There is a schizoid quality to the idea and practice of merit in federal employment. It began with the rejection by right-thinking citizens of politics as the criterion for public employment. Yet, Americans also distrusted the possibility of government removed from political responsibility.

By *merit* the Pendleton Act of 1883 meant nothing that would hint at elitism. Rather, the civil service began with the assumption that federal employment should require practical knowledge and job-related skills. At the same time, merit-based employment was introduced not in one fell swoop but incrementally, over a small percentage of federal jobs, and expanded through executive orders. Thus, the expansion of the merit system depended upon presidents' political incentives for expansion (or for the occasional exemption of positions from protection). For example, Franklin Roosevelt excluded the employees of New Deal agencies from merit protection, enabling him to use those agencies for patronage. However, at the end of the 1930s, with the new agencies fully staffed by Roosevelt loyalists, the president blanketed those positions into merit protection. In short, politics was never far removed from the merit system.

The central story of the merit system from 1883 through World War II is its expansion across the breadth of federal employment. In the postwar era up to the present at least two main themes have been introduced. First, the relationship of high-level career administrators to political officials has become increasingly tangled. Modern government's operations depend on careerists with specialized skills and career-long associations with agencies. Yet, political responsiveness dictates government by political appointees. Presidents fill about three thousand appointive positions in the executive branch. For over four decades reformers and participants have searched for a mechanism to rationalize the relationships of upper-level careerists and political appointees. The "super-grade" ranks of the Civil Service were established in 1949. President Eisenhower designated Schedule C-exempted positions to accommodate policy-sensitive roles within the career service. The second Hoover Commission proposed a senior civil service as a way of bringing careerists into the upper levels of the executive branch. Finally, the Senior Executive Service was created in 1978 to provide the flexibility and incentives thought necessary for creating stable relationships between senior careerists and political executives. Yet, Ingraham observes, the reality of governance within the contemporary executive branch is driven by the presidential agenda. What reform attempted to stabilize is eroded by politicization.

A second theme swirling through the public service in recent decades is the inability of the old classification system to meet new challenges, ranging from expanded and more complicated job roles to the demands of a changing society for fairness and equality in hiring, evaluation, and promotion. In the federal merit system, job dictates rank, and the civil service is a hierarchy of job ranks. The old classification system has become a straitjacket into which an increasingly complex federal work force is stuffed. If classification systems should encourage career development, enhance performance assessment, and reward efficacy, the current system fails. Yet, here too politics constrains change. From the public employee unions to Congress and the White House, one finds incentives for defending the current organization of the merit system. Reform may seem remarkably attractive and necessary, but it comes at a cost to those whose political leverage over the public service presumes the existing personnel system.

There are two concerns that might have been more clearly addressed by Ingraham. In contrast to the United States, other advanced democratic societies exhibit clearer and more stable relationships between career administrators and elected politicians. Because this book is properly concerned with the relationship of the public service to the American political regime, the addition of a comparative perspective would have clarified some of the structural and cultural reasons for problems it addresses. Additionally, might the current confusions over public service reform be more than problems in our understanding of the merit system? Throughout her discussion of the prospects for meaningful reform, Ingraham seems to suggest that the barriers to reform lie in our failure to think sufficiently clearly about merit and the public service. However, in a perceptive observation she implies a deeper cause for our malaise: "The fit between values that the polity deems it desirable to pursue and those reflected in its public service is critically important" (p. 140). Perhaps, more than increasing the clarity of our view of personnel policy, what successful public service reform requires is a fundamental agreement among Americans about what government ought to do and how those purposes should be pursued.

These are minor qualifications about a very good book. *The Foundation of Merit* both surveys the development of the protected public service and critiques the current agenda for public service reform. It is successful on both counts, offering an informative and thoughtful introduction to the American public service. This book fills a gap in the literature of American government, and with it Ingraham performs a real service for scholars, teachers, and students of American politics.

**Can Democracies Fly in Space? The Challenge of Revitalizing the U.S. Space Program.** By W. D. Kay. Westport, CT: Praeger, 1995. 256p. $59.95.

Radford Byerly, Jr. *Boulder, Colorado*

Early in the space era Defense Department official Herb York said, "Space is a place, not a mission." This remark epitomizes a continuing tension in the space program. On the one hand, people like York believe the United States should enter space when it is the best place to accomplish a purpose. For them, space is a means. On the other, there are those who believe space exploration is a self-justified mission, an end in itself. This tension is key to understanding W. D. Kay's book, which finds that the U.S. space program is in serious trouble and that "the source of [its troubles] is democracy itself" (p. 28). If space is a means to a national goal, its effectiveness and use of resources should be evaluated, and it should be democratically accountable. If space is self-justified, one might argue, as Kay does, that the government should "give the program the resources it needs ... be patient with its mistakes, and ... protect it from swirling economic, social, and political forces" (p. 8). In other words, the program need not be democratically accountable.

Kay clearly believes space is a self-justified mission. He declares himself a "devoted follower" of the television show *Star Trek* in his first sentence (p. ix), and his argument flows from this point of departure. For example, he deduces that the Trekkers' fictional parent society long ago had "made *and followed through on* a decision ... to devote considerable ... resources to exploring and developing space" (p. ix). He believes the United States should do the same and that after the Kennedy commitment of large resources to go to the moon we have since faltered. Through the lens of his enthusiasm for space, Kay misreads the history in two ways. First, the Apollo mission to the moon was intended as a demonstration to the Soviet Union and the world that, even though the Soviets had beaten us into space, we were still the world's technological leaders and could drop nuclear warheads wherever we wanted. Second, while Kennedy's commitment was large, it was limited to the moon race.

Kay's thesis is that the U.S. government as it has operated for the last three decades cannot conduct an adequate space program and that it must be reformed, not NASA. Given his premises, his argument is clear. Chapter 1 describes the problems, including cost overruns, spacecraft failures, program delays, and unenthusiastic political support. He discounts NASA's many successes, especially in space science, and the multibillion dollar appropriations it continues to receive. He decries "steadily declining budgets" during the 1980s (p. 5), although his own table shows that NASA budgets increased (p. 57). Chapter 2 describes and rebuts standard explanations for the problems, including NASA's technical and political incompetence and lack of presidential leadership. The middle three chapters apply different theoretical perspectives to various aspects of the program and together find that the traditional workings of democracy, including annual appropriations and the involvement of interest groups, exacerbate the features of major NASA projects that make them difficult in the first place—size, cost, and complexity. Chapters 6 and 7 examine and reject solutions others have proposed, such as more international collaboration, commercialization, and revolutionary technologies. In the final chapter Kay raises the possibility that "no viable solution exists at this time" (p. 192) and that democracies can fly in space only if they are willing and able to find a new modus operandi.

Throughout, Kay portrays NASA as substantially blameless because it is essentially powerless. For example, the shuttle program "was shaped from the start by larger political forces" (p. 59). From this perspective cost overruns just seem to happen. However, I would argue for an alternative view of the situation. Opinion surveys continue to show popular support for a space program. Given this base of support, NASA could discard its traditional procedures and develop and execute a vital space program within existing democratic processes.

A more democratic and workable alternative program can be illustrated through the space station program, which has spent billions of dollars and still has nothing in orbit years after the first flight was promised. Kay recognizes that as soon as the program got under way it "became clear that NASA had promised more than it could deliver" in terms of schedule, electric power, maintenance, and capability (p. 18). As these problems came to light, "NASA began redesigning the station (several times)" and "its anticipated costs continued to rise" (p. 18). The current design is more expensive, smaller, and less capable than what was originally promised. Kay overlooks an important fact: NASA's earlier design was virtually impossible because it was essentially creationist, jumping from short duration shuttle flights to a full-blown station with no intermediate steps. Based on little experience in human spaceflight, NASA proposed to design the whole station in advance, build it, and launch it in about twenty sequential shuttle payloads for assembly in orbit. As the complex design was not separable into parts, it required a huge, open-ended commitment and insulation from democratic processes.

An alternative evolutionary approach, compatible with democratic processes, would proceed incrementally: learning more from shuttle flights, extending the in-orbit time of the shuttle, building unpiloted and piloted platforms released from and retrieved by the shuttle, putting up a capsule that could remain in orbit between shuttle flights, and so forth, to gradually accumulate (and learn from) experience. The results at each step would justify the next step and the necessary incremental funds. Such an evolutionary approach would earn its way, obviating both a blank-check commitment and exemption from democratic accountability. But an evolutionary approach is not NASA's style, and in Kay's analysis change of NASA is not an option.

In sum, the book is flawed at several levels. It illustrates the kind of blinkered thinking that motivates much science and technology policy analysis. Finally, Kay's analysis is based largely on the trade press, and the text was inadequately proofread.

**Presidential Polls and the News Media.** Edited by Paul J. Lavrakas, Michael W. Traugott, and Peter V. Miller. Boulder, CO: Westview Press, 1995. 275p. $65.00.

S. Robert Lichter, *Center for Media and Public Affairs*

This is a book that fails in the task it sets for itself and yet succeeds in a larger intellectual sense. The editors have assembled a collection of essays by media scholars and opinion researchers for news organizations. The various articles are generally of high quality and are integrated well into the book's conceptual scheme, which is presented in a concise but comprehensive introductory essay. In a mission statement, the editors proclaim their proactive and reformist agenda to increase the educational function of media election polling. Therefore, the volume is "aimed first and foremost at journalists" (p. 5) and only secondarily at scholars and other campaign observers.

Essentially, this book is intended as a tool to help journalist-pollsters improve their product by incorporating scholarly insights into their news stories. Alas, this intention is undermined by a dryly academic style, frequently technical content, and sometimes unrealistic reform proposals. (A more accessible volume, to which Traugott also contributed, is Mann and Orren's *Media Polls in American Politics* [1992].) But these weaknesses should not deter social scientists interested in a state-of-the-art explication of the practices, pitfalls, and potential of media election polling.

The volume is well organized, with successive sections on past developments in election polling, current methodological issues, case studies of 1992 media polls, and the public impact of election news. Inevitably, some selections are weaker than others. Considerable data and methodological detail are devoted to demonstrating some unremarkable findings: preelection polling news increased sharply in 1992; it is unwise for pollsters to assign *all* undecided election-eve voters to the out-party challenger's column. For the most part, however, the case studies are highly informative, the critiques of current practices insightful and the suggestions for improvement provocative.

The topics range from narrowly technical (but hardly trivial) questions about data collection and analysis to broad issues of how news judgments might be better grounded in the scholarly literature. Also of interest are Andrew Kohut and Larry Hugick's compilation of Times Mirror's indispensable polls on public attitudes toward the news media and Diana Mutz's perceptive examination of how horse race reporting influences the dynamics of candidate fund-raising. Mutz's analysis is especially important for focusing attention on the campaign "preseason," when journalistic discretion and the potential for unintended media effects may be greatest.

The centerpiece of the volume is a series of essays in which media pollsters describe their own efforts in 1992, and they and other researchers present suggestions for future improvements. For example, Warren Mitovsky and Murray Edelman directed polling by Voter Research & Surveys (VRS), which the major television networks created to conduct exit polls and election projections. On election day, VRS data were distributed to participating news organizations, each of which was responsible for its own interpretation. The authors unflinchingly document the difficulties of conducting and reporting these surveys under heavy time pressure, the errors that inevitably arise, and the ways this information can distort the election process. For example, leaks of preliminary results during the 1992 primaries affected both the stock market and the candidates' get-out-the-vote drives.

The *New York Times'* Michael Kagan describes that paper's 1992 polling operation and uses the data to review the dynamics of voter opinion throughout the presidential campaign. Kagan properly focuses on voter rejection of the incumbent's economic stewardship as the key to the outcome. He also calls attention to the gap between subjective perceptions and objective conditions. Unfortunately, he fails to address the arguments made by Lipset (1993), Ladd (1993), and Patterson (1993) that voter perceptions of the economy (and hence of Bush's leadership) were distorted by overly gloomy media coverage. Thus, the media remains the "dog that didn't bark" in Kagan's treatment of preelection opinion.

The most illuminating of these participant-observer accounts comes from the *Washington Post's* Richard Morin. He provides a detailed insider's perspective on media polling, which covers both the successes and the failures of the *Post*/ABC polling operation. Morin's assessment of the media's performance is that the quality of polling improved in 1992, while the reporting "remained data-rich but analysis-poor" (p. 124). This is also an underlying theme of two essays by Traugott. One of these argues for the integration of focus groups with survey research. The other, co-authored by Jennifer Means, makes the laudable point that the media's attention to marginal data often obscures more interesting (and newsworthy) relationships between variables. However, it also illustrates the ambiguities involved in putting this admonition into practice.

Traugott and Means revisit a *Washington Post* polling story on political fallout from the Gennifer Flowers scandal. Whereas the *Post* emphasized the danger to Clinton's candidacy, Traugott and Means note that (not surprisingly) public concern was lowest among likely Clinton voters and highest among those not predisposed to support him. They argue rightly that attention to these differences would have alerted reporters and readers alike to Clinton's resiliency. But Traugott pushes the point too far by complaining that "the media should have played [the story] more for what it was: partisan politics as usual" (p. 17). An issue of personal character is not reduced to partisan politics simply because it is perceived selectively according to audience predisposition.

More generally, expertise in survey analysis is no guarantor of "correct" interpretation on topics that engage political commitments. The desire for a more informed journalism guided by "experts" has animated media critics from Walter Lippmann to the Hutchins Commission. It is echoed in the present volume by Mitofsky's suggestion that journalists defer to staff researchers in interpreting polling data. This long-standing debate is well worth revisiting at a time of changing journalistic standards, and this book succeeds in rekindling and advancing the debate.

**Political Judgment: Structure and Process.** Edited by Milton Lodge and Kathleen McGraw. Ann Arbor: University of Michigan Press, 1995. 313p. $39.50.

James H. Kuklinski, *University of Illinois at Urbana-Champaign*

In 1989 Bob Luskin, John Bolland, and I wrote an article in the *American Political Science Review* that raised questions about schema theory and the empirical measurement of schemas. Milton Lodge and Kathleen McGraw, among others, responded to our critique in the same issue of the journal. Lodge and McGraw provide an even more compelling response in their edited volume, *Political Judgment*. In a very real sense, this book can be interpreted as a "we'll-show-you" reply (which is not to suggest such intent on the editors' part).

Indeed they have shown us. The editors note early on that none of the chapters uses the term *schema* (which is *almost* true), but "the *concept* of schema as an organized memory structure is still vitally important to the understanding of political reasoning and judgment [even though] the particular form such structures may take demands more precision than the ubiquitous schema term provides." So despite the absence of the term *schema*, mental structures and processes are what this book is all about; and what a good book it is. This collection of essays, more than any other currently available, convincingly demonstrates the value of a structural/processional approach to the study of mass attitudes.

The book is also the best statement to date of the Stony Brook perspective on the psychology of voting and public opinion. But it is more. To the credit of the editors, the volume includes chapters that actually challenge some of the Stony Brook findings. These include, especially, the chapters by Lau and Rahn.

The introduction serves not only as a summary of the chapters that follow, but also as a useful integration of the reported findings. Especially impressive is the section that lays out directions for future research. It identifies questions on which there is not yet agreement and thus functions as an abbreviated roadmap that some political psychologists will undoubtedly follow. In the best tradition of scientific inquiry, the editors emphasize that researchers should not try to resolve outstanding controversies in either-or terms; rather they should identify the conditions under which one or another finding holds. This is sage advice.

It will be impossible to give the various chapters their just due. Most of the substantive discussions are nuanced, and many of the authors present new and innovative methodologies (itself a reason to read the book). Among those authors who focus specifically on structure (as opposed to process), Rahn, Berent and Krosnick, and McGraw and Steenbergen all employ the adjusted ratio of clustering (ARC). The ARC measures the extent to which people's recall of information from the same category follows a sequence that differs from chance. The higher the ARC, the greater the mental organization within that category.

This is a simple but nifty measure of structure. If there is a problem, it is that the researcher must define the categories a priori. This means different researchers almost certainly will identify different categories. Thus, for example, Rahn uses person-focused versus attribute-focused; Berent and Krosnick, agreeable knowledge versus disagreeable knowledge; and McGraw and Steenbergen, policy versus personal. This leaves the obvious question: What exactly is the real mental structure? All of the above? One of the above? None of the above? Of course, the investigator always imposes some kind of order when conducting attitudinal research; the problem just seems to be more self-evident in the case of mental structures.

That said, each of these pieces, along with the Judd and Downing chapter on stereotypes, offers a glimpse into how people organize political stimuli in their minds. Berent and Krosnick, moreover, use four distinct methods to substantiate their findings. Those who teach research methods might wish to include their chapter as a model that we all should strive to emulate.

If I have any problem with these chapters, it is that a couple of them make fairly strong statements on the basis of rather weak data. Significance levels sometimes fall by the wayside.

The chapters on process raise more provocative questions. The on-line processing model so closely identified with Stony Brook raises an especially interesting challenge. If that model is correct and if one is willing to take its implications to their logical conclusion, survey researchers could be out of business. Survey research by its very nature assumes memory-based decision making. But the on-line processing model—the Lodge chapter lays it out in detail—says we do not remember specific political information. Thus far the Stony Brook researchers have not been willing to take the implications to their extreme, and survey researchers typically give the on-line model token acknowledgment and then do what they always do. Perhaps this coexistence will continue. But then again, perhaps an enterprising graduate student will take on what is most certainly an interesting challenge.

If he or she does so, one good starting point will be Feldman's chapter on answering survey questions. He begins to grapple with the seeming incompatibilities by suggesting that on-line processing will occur primarily when affective evaluations are involved. This sophisticated discussion suggests that the resolution might lie with a recognition that either on-line or memory-based processing can occur, depending on the circumstances. If so, we have a long way to go to identify them. (Feldman's chapter also serves as a reminder that referring to the Stony Brook "school" masks the variety of research going on there.)

The Stroh, Lau, Boynton, and Taber and Steenbergen chapters report some of the most creative and interesting research in contemporary political psychology. Stroh effectively uses reaction times to ascertain how people balance effort minimization and accuracy maximization. Lau employs an imaginative scrolling technique to identify how people choose what kinds of information and under what conditions. Boynton and Taber and Steenbergen combine experiments and computational modeling to try to uncover the actual processes people use to make judgments. Each of these chapters reports research in progress and all the authors explicitly note some of the complications that lie before them.

These research efforts also leave the impression that political decision making is even more complex than we have been wont to recognize. This raises three related questions: Just how complex will our future models be? Is the next logical step to draw on neurology, chemistry of the brain, and the like? How will we keep our eyes on politics?

All in all, then, *Political Judgment* not only represents state-of-the-art research in one domain of political psychology; it also encourages us to think about the nature of future research. Any book that can do that is a success.

**In Pursuit of the White House: How We Choose Our Presidential Nominees.** Edited by William G. Mayer. Chatham, NJ: Chatham House, 1996. 368p. $17.95.

Ryan J. Barilleaux, *Miami University (Ohio)*

In the old story about several blind observers and an elephant, each one who touched a separate part of the beast concluded that he had encountered a different kind of animal. This parable comes to mind when considering discussions of American presidential politics. Journalists, focusing on the latest election, poll, candidate, issue, or controversy, tend to concentrate on what is unique in each election. They highlight the horse race aspects of presidential campaigns and, as good race callers do, make the contest as dramatic and exciting as possible. Academics, however, often focus their attention on the constants, regularities, and recurring patterns of national politics.

As a result, these divergent approaches often lead reporters and scholars to talk past one another. Certainly, journalists will question researchers about some of the more arcane aspects of the presidential selection process, but reporters tend to favor political practitioners or other reporters when choosing counterparts with whom to discuss electoral politics. Academics often dismiss journalists as too concerned with the ephemera of campaigns and retreat to their regression equations for consolation.

Into this mix comes this latest survey of presidential nomination politics. William G. Mayer has assembled a collection of research essays on various aspects of nomination process and politics, providing readers with a good overview of the process and its operations. Mayer and his colleagues have not exactly bridged the traditional gap between scholars and journalists, but their work can help students of presidential nominations sort out what is recurrent and what is ephemeral in elections. They cover such topics as structures of the process (primaries, caucuses, campaign finance), important participants (activists, interest groups, front-runners), and continuing issues (forecasting nominations, party reform).

It is commonplace to describe an edited volume as uneven, and this one is no different. Some of the chapters offer information and perspective essential to anyone who would understand presidential selection. For example, Emmett H. Buell's chapter on the "invisible primary" (what *Congressional Quarterly* and others call the "exhibition season" that precedes actual primaries and caucuses) is a fascinating and informative study of this poorly understood phase of the selection process. Likewise, Mayer's chapter on the operations and significance of caucuses helps readers appreciate this selection device. The majority of Americans will never take part in a caucus, yet these events can be significant to the outcome of the race. And Emmett T. Flood and Mayer's chapter on third-party and independent candidates helps to put discussions of a Perot, Powell, Jackson, or Buchanan independent candidacy into perspective.

Other chapters are less useful. Mayer's chapter on forecasting presidential nominations offers much ado about very little. He concludes that nominations are at least partially predictable, but then offers such vague and generic statements about predictability that he ends up with the kinds of platitudes and statements of the obvious that pass for insights on the Sunday morning talk shows. Duane Oldfield's chapter on Christian conservatives is fine as far as it goes, although it reveals a number of the author's prejudices: the doubly capitalized (and implicitly demonic) term *Christian Right* is used throughout and the author insists on using all four of George Bush's names, as if there is some political significance in having two middle names. Why devote a chapter only to one group in presidential politics? What about African Americans, women, labor, and others?

Since the subject has now moved to what is left out of this book, it is interesting that candidates are minor characters in most chapters of the book. Chapters focus on how the press treats front-runners, how candidates deal with Christian conservatives, how activists affect nominations, and how different structures shape the environment in which candidates operate. However, only two chapters make candidates the centerpiece of their analysis: Buell's essay on the "invisible primary" and Flood and Mayer's chapter on third-party and independent candidates actually focus on candidates.

Despite this gap and the other signs of its unevenness, *In Pursuit of the White House* does offer a helpful—albeit incomplete—analysis of the presidential nomination process. It is useful for courses on campaigns and elections, graduate courses on electoral or presidential politics, or for those scholars who want an update on the current nomination system. Used in conjunction with other works on nomination politics—such as Polsby and Wildavsky's *Presidential Elections* (1996) or Wayne's *The Road to the White House* (1984)—it can contribute to a better understanding of how the United States gets its presidential nominees.

**Mexican American Youth Organization: Avant Garde of the Chicano Movement in Texas.** By Armando Navarro. Austin: University of Texas Press, 1995. 288p. $40.00 cloth, $16.95 paper.

Benjamin Marquez, *University of Wisconsin-Madison*

One topic in critical need of scholarly attention is the role that Mexican-Americans played in the Civil Rights movement. The political upheaval that occurred on university campuses and in Mexican-American communities throughout the Southwest during the 1960s—commonly referred to as the Chicano Movement—was a complex phenomenon that has yet to be untangled. Organizations usually considered a part of the Chicano Movement include a wide range of groups such as the United Farm Workers Union, the Brown Berets, the Crusade for Justice, and La Raza United Party. A number of books have been published on the Chicano Movement, but most are journalistic accounts that do not place the period in the larger context of American history or deconstruct its political meaning. Armando Navarro's *Mexican American Youth Organization* is the latest attempt to deepen our understanding of this period through a case study of one of its most controversial organizations.

Navarro's book tells the story of the Mexican-American Youth Organization (MAYO) from the time it was formed in 1967 by students at Saint Mary's University in San Antonio, Texas, to its dissolution five years later. Throughout its short and volatile existence, the group challenged Anglo domination in Texas with a combination of conventional pressure group tactics and social disruption. Many MAYO activists went on to become the most visible leaders of the Chicano Movement, extending their political influence through social service agencies, law firms, and universities long after the group's demise. MAYO's uncompromising politics and brazen leadership tempts one to draw parallels between it and black civil rights organizations. Are these comparisons justified? Was MAYO the Mexican-American equivalent of the Student Nonviolent Coordinating Committee? Was it a vanguard organization that broke down the structure of racial control in the Southwest? Unfortunately, answers to these and other critical questions concerning race, ethnicity, and social mobilization are not found in this work.

*Mexican American Youth Organization* contains all the problems that marred most books written during the 1960s and 1970s on the Chicano Movement: The lack of a coherent theoretical structure, rhetorical excess, and a narrative mired in detail. One reason it resembles this older body of literature is that virtually all of the data for the book were drawn from research conducted in 1973 for the author's doctoral dissertation. Navarro cannot be faulted for tapping a good source of historical information, but little effort was made to incorporate more recent literature in Mexican-American politics or social movements theory into his book. The two introductory chapters on the Chicano Movement cite only a handful of works published after the mid-1970s. Likewise, an examination of the endnotes for the substantive chapters reveals that little additional research was added to the original study. The dated literature, lack of theoretical structure, and close attention paid to obscure partisan conflicts mark this as a book that did not reach its full potential.

This lack of scholarly rigor is all the more frustrating because Navarro's data are often richly suggestive. For example, it is clear that his case study of MAYO could have informed the debate over the relative salience of race, class, and culture in American politics. MAYO was promoting a quintessential Chicano Movement agenda, one that contained elements of socialism, cultural nationalism, and racial separatism. The group also received support from a large number of poor Mexican-Americans, secured concessions from school boards and local governments, and eventually formed an independent third party that overthrew Anglo-dominated political regimes in several south Texas counties. Navarro attributes these accomplishments to the power of racial and cultural solidarity but offers no systematic analysis to support this contention. His own research documents significant opposition to MAYO from Mexican-American elected officials and community leaders, a clash over political and economic values that other studies suggest can compromise ethnic group unity.

Navarro documents another example of intragroup conflict in his account of the battle between MAYO and San Antonio's congressional representative, Henry B. Gonzales. By the time MAYO appeared on the scene, Gonzalez was a well-established critic of American race relations with a long record of civil rights activism in the Mexican-American community. Gonzalez opposed everything MAYO stood for. He accused the group of reverse racism, squandering public monies, and disloyalty to the United States. He also mounted a sustained public attack on MAYO and worked to terminate its participation in social service programs offered by federal agencies like the Office of Economic Opportunity. Finally, Congressman Gonzalez pressured tax-exempt organizations such as the Mexican American Legal Defense and Educational Fund and the Ford Foundation to stop supporting MAYO's activities. Gonzalez's crusade against MAYO eventually destroyed the group and defunded the Chicano Movement, but Navarro leaves the larger significance of this dramatic episode unexamined.

This book is seriously flawed. However, because of the subject matter's importance, *Mexican American Youth Organization* will be of interest to researchers in the field of minority politics. MAYO was one of the earliest in a series of Chicano Movement organizations whose activities changed the shape of American politics. There are enough tantalizing empirical nuggets contained within this study to demonstrate that Mexican-Americans were key players in the Civil Rights movement and that further research on the subject is in order.

**Narrative Policy Analysis: Theory and Practice.** By Emery Roe. Durham: Duke University Press, 1994. 199p. $39.95 cloth, $15.95 paper.

Charles Lester, *University of Colorado at Boulder*

Emery Roe hopes to illustrate how policy analysts would benefit from applying literary theory to intractable policy problems. As Roe sees it, traditional techniques of policy analysis often merely aggravate such problems because of persistent empirical, political, legal, and bureaucratic uncertainty. Thus, "the only things left to examine are the different stories policymakers and their critics use to articulate and make sense of . . . uncertainty, complexity, and polarization" (p. 3).

The first step of "narrative policy analysis" is to identify the story(ies) that "underwrite" and "stabilize" the assumptions for policymaking in any given context. For example, the classic story of "the tragedy of the commons" often justifies or enables policy interventions to manage common resources. Step two is to identify the narratives that do not conform to the definition of a story (having a beginning, middle, and end) or that run counter to the dominant narrative. A piecemeal critique, for example, is usually a "nonstory" because it does not present a logically complete alternative for action. The heart of narrative policy analysis lies in step three, which requires the generation of a "metanarrative" told by the comparison of two stories. The governing precept is "semiotics and gestalt psychology, namely, a term is defined by what it is not" (p. 4). To illustrate, Roe shows how a dispute about aerial pesticide spraying in California might have been more effectively addressed had the parties refocused on the metanarrative of public risk perception—an idea that was not coherently featured in any of the stories at play.

Through additional case studies, Roe elaborates on techniques for constructing metanarratives and on the fourth step of narrative policy analysis: the determination of whether the metanarrative "recasts the issue in such a way as to make it more amenable to decision making and policymaking" (p. 4). For example, he shows how the use of a computer-based narrative "network analysis" of the various problem definitions of an irrigation controversy helped expose a metanarrative of internal agency conflict between the goals of environmental and agricultural protection. This metanarrative was then more accessible to traditional organizational analysis. Later chapters take the reader deeper into literary techniques to discover the metanarratives of the global warming and Native American burial remains controversies. The arguments become increasingly subtle and difficult, relying on such ideas as "sociolect," "idiolect," "ungrammaticality," and "intertextuality."

*Narrative Policy Analysis* has much to offer in that it both presents innovative methods and continues the "policy as argument"-based challenge to modern technocratic policy analysis. Moreover, in contrast to many "postmodern" perspectives, Roe is interested in the *reconstruction* of policy controversies to make them more actionable. For Roe, this requires the embrace of "tolerance" and strategies that give voice to marginal interests in the policy process: "Tolerance is here a methodological, not normative, imperative" (p. 12). Roe also places great confidence in literary technique, and he observes that while practitioners may appreciate the informational value of stories, "what has been by and large missing until *Narrative Policy Analysis* is a way of channeling this appreciation into better managing or solving concrete policy problems" (p. 9). Roe's concluding recommendation is "to design and establish joint degree programs [between humanities departments and policy schools], leading to a specialization in 'applied narrative analysis'" (p. 152). The common theme of such programs should be the literary critic's precept that "the act of interpretation can be defended" (p. 153).

This theme, of course, dates back at least to Weber, and we have a rich history of interpretive social science that expands upon it; indeed, the tension between "sociolect" and "idiolect" was arguably captured in Weber's discussion of the relationship between "concept and reality." In short, while Roe is probably correct about the deficiencies of the modern school of public policy, this reader is driven to ask whether his recommendation is the best way to reintegrate the insights of interpretative social science into the practice of policy analysis. For example, in the policy sciences tradition articulated by Lasswell and McDougal the special role of the scholar is to "clarify and identify for the different participants in community process the common interests which they themselves may not have been able to perceive" (*Jurisprudence for a Free Society*, 1992, p. 23). This is done by applying a set of comprehensive concepts to the total social process at issue, including systematic inquiry into the relevant actors, their perspectives and structural relationships, and their strategies. Significantly, Lasswell and McDougal thought it essential to elaborate the role that "myth" plays in the structuring of a social context. In other words, what are the fundamental stories that *underwrite*, to use Roe's term, a pattern of policy decision or action? The elaboration of myth, though, is just one piece of attempting to construct a *comprehensive* map of a policy context. Although Roe acknowledges the importance of understanding "power and politics," he subordinates them to their articulation in the narrative—a strategy that may weaken the effort to produce grounded, realizable recommendations.

The policy sciences tradition is also consciously interpretive and thus it emphasizes the importance of the analyst clarifying her own observational standpoint—what are the basic value commitments animating the analyst's interpretation? Although the "reader" is obviously central to literary theory, *Narrative Policy Analysis* says too little about how the reader should understand what he is doing in relation to the narratives being discovered and reconstructed. The point of such clarification is to be as reflexive as possible about potential biases underlying any given narrative, including the analyst's own metanarratives.

Roe's approach also requires complexity, uncertainty, and polarization because without such ambiguity and difference there is no literary tension to deconstruct. For those "many policy issues" that are not complex, uncertain, or divisive, "you can get along quite well with conventional analysis alone" (p. 160). Aside from the difficulty of distinguishing the complex, uncertain, and divisive policy issue from the simple, clear, and agreeable one, this conclusion seems to unnecessarily limit the fundamental insight that social science is interpretative. Shouldn't we expose future analysts to this broad epistemological point as a tenet of good practice, for example, as is suggested by the clarification of observational standpoint in the policy sciences?

Finally, if our goal is, as Roe states, to advance the profession of policy analysis by embracing useful methods, we may need to think about how to translate such concepts as *sociolect* and *intertextuality* into more useable concepts for the practitioner. Instead of looking for "stories," why not analyze arguments and ask if they make sense? Rather than creating metanarratives by identifying "ungrammaticalities," why not construct alternatives by identifying interests common to multiple participants? The challenge, it seems, is not to make policy analysis more like literary theory but to make the insights of literary theory, and interpretive social science more broadly, accessible to the practitioner. In the end, Roe's text is an important contribution to this metanarrative about how to improve our ability to reconstruct policy controversies in a socially useful way.

**Flirting with Disaster: Public Management in Crisis Situations.** By Saundra K. Schneider. Armonk, NY: M. E. Sharpe, 1995. 187p. $55.00 cloth, $21.95 paper.

Louise K. Comfort, *University of Pittsburgh*

With the waning of the Cold War, governmental response to disaster has emerged as a major issue in public policy. Natural disasters have historically been considered acts of God, unavoidable events treatable only through lament. However, a recent series of costly hurricanes, earthquakes, and floods have contributed to the increasing visibility of response to disaster as a measure of governmental performance. Saundra K. Schneider's *Flirting with Disaster: Public Management in Crisis Situations* captures some, but not all, of the issues involved in this dynamic process.

Schneider's study makes several valuable contributions to the public policy literature. She nicely reviews a substantial body of disaster research, much of it done in the 1980s and early 1990s. Her discussion of "emergent norms" suggests but does not explicitly articulate the more interesting concept of *self-organization*, or the reallocation of energy and action in response to urgent disaster. Schneider's book clearly moves away from treating disaster management as a standard problem of administrative command and control and toward interpreting it as a more flexible, adaptive, complex system. Such a characterization represents a redefinition of disaster management as a continuing process of organizational learning in public policy.

Schneider questions why disasters are viewed as legitimate public problems requiring governmental action and asks what that action should be. She organizes her study into three parts: an analytical and theoretical framework for assessing governmental response; case studies of actual governmental responses following natural disasters; and a brief summary and interpretation of her findings, with recommendations for change.

Schneider's analytical framework evaluates governmental responses to different types of disasters according to their perceived success or failure, as determined by the size of the gap between "bureaucratic norms" and "emergent norms" in the public's assessment of governmental performance. In her investigation of actual instances of governmental response to disaster, she identifies six disaster situations that required federal action and ranks them on a continuum from failure to success. She uses reported public perceptions to determine the "gap" between bureaucratic norms guiding governmental practice and emergent norms of expected behavior arising from the affected population. To establish the existence of this gap, Schneider relies on secondary sources, such as reports from professional administrative organizations and other research, as well as some interviews with disaster managers, chiefly at the federal level. None of these sources, however, provides a rigorous characterization of governmental action in any of the six disaster contexts. Instead, her argument rests on the public's perceived expectations of what the federal government should do. This analysis does not address the responsibilities of communities in vulnerable areas or acknowledge the potential of citizens to mitigate risk.

Regrettably, Schneider restricts her inquiry to the government's role in providing relief after a disaster has occurred. This design omits wider issues of managing risk in a complex, changing society and portrays governmental response largely outside of its operating context of intergovernmental dependencies, shared responsibilities, limited resources, and an expanding and increasingly diverse population living in vulnerable areas. Furthermore, her argument masks the basic weaknesses in U.S. disaster management—lack of informed risk assessment, reduction of vulnerability, and informed preparedness for natural disaster at the local level, supported by appropriate technical and administrative assistance as well as judicious mobilization of resources from state and federal levels. Disaster management is fundamentally an intergovernmental process, and any perceived failure most often reflects a breakdown in this complex process of intergovernmental communication and interdependent action. Such breakdowns illustrate the inability of one level of government to make a timely transition from routine public services to communitywide protection of citizens in a time of disaster, thereby limiting the capacity of other levels of government to function in supportive roles.

The weakness of the perceived gap as a measure of effective disaster response is evident from Schneider's analysis of the success or failure of governmental response at six different locations: three sites affected by Hurricane Hugo in the Caribbean and the East Coast, followed only weeks later by the Loma Prieta earthquake on the West Coast in 1989, and two sites affected by Hurricane Andrew in 1992. Her analysis treats governmental response at these six sites as independent cases, when in fact federal response to the four 1989 sites was highly interdependent, as it was to the two 1992 sites. The analysis assumes that little or no gap between public expectations for governmental response and bureaucratic norms for disaster response constitutes success, while a large gap between the two constitutes failure. This argument fails to distinguish the effects of different levels of local awareness of risk, preparedness for disaster, and capacity for action at these six sites, or the consequences of simultaneous demands from distant locations upon the federal response system in assessing the validity of public expectations or the actual capacity for federal response. Perception of the gap may be flawed by faulty reporting, inadequate communication, or uninformed citizens. Measures that compare the initial conditions of disaster-affected communities to the system's actual capacity and actions taken are likely to be far more accurate in assessing the effectiveness of governmental response to disaster.

Schneider does not address some issues that have a significant impact upon the role of governmental response in disaster. These include topics linked to the rapidly evolving role of information technology in facilitating interactive communication and information exchange within and among organizations engaged in disaster operations. These processes prove most vulnerable to failure under the dynamic, urgent conditions of disaster. Integrating this technology into disaster operations across jurisdictional levels will significantly increase governmental capacity for response and enable informed community action. Significant steps have already been taken in this direction by FEMA, for example, in response to the 1993 midwest floods and the 1994 Northridge earthquake, cases mentioned only in passing Schneider's study.

The potential disruption and high costs arising from natural disasters in interdependent, densely populated areas warrant careful and sustained study. Flippant avoidance of risk assessment and disaster planning in administrative practice, captured in the provocative title of Schneider's book, underscores the importance of the effort to redefine natural disaster as a continuing public policy issue.

**Democracy and Technology.** By Richard E. Sclove. New York: Guilford, 1995. 338p. $42.00 cloth, $18.95 paper.

Sheila Jasanoff, *Cornell University*

The second half of the twentieth century has produced its share of influential voices decrying the impact of technology on political life. Work in this vein ranges from the sweeping critiques of modernity, and its obsession with instrumental rationality, by social theorists such as Jacques Ellul, Jürgen Habermas, and Ulrich Beck to more precisely centered attacks on technology's destructive, gendered, and hegemonic aspects by critics such as Rachel Carson, Donna Haraway, and Vandana Shiva. With less individual panache, but no less powerfully in the aggregate, a generation of historians and sociologists have unpacked the black box of technology, exposing dense interconnections between the human and nonhuman components of technological systems and shattering our placid assumptions that machines are unproblematically passive or morally neutral. This wealth of scholarship has liberated the social sciences from the seductive polarities of technological and social determinism and directed inquiry toward the complex divisions of agency and power between human beings and their not-so-subservient technological creations.

Richard Sclove, executive director of the Massachusetts-based Loka Institute, is concerned less with the theoretical aspects of the expanding literature on technology than with the implications of technology's socially and politically contingent character for democratic politics. His aim in this book is to inform ordinary citizens about the social dimensions of technology, so that they, in turn, can further the egalitarian and participatory principles of "strong democracy" (pp. 25–6) through their technological choices. Elaborating on this theme, Sclove serves up a curious blend of primer and polemic. Charts and tables reminiscent of a consultant's slide kit punctuate the text, conveying the impression that even seemingly intractable technological developments are amenable to social control. All one needs is an awareness of the problem and the will to follow Sclove's step-by-step instructions on how to reintroduce democratic principles into one's interactions with the mechanical world.

The first half of the book presents a refreshingly lucid and readable digest of many different strands of scholarship to establish the point that technologies are *social* structures, inasmuch as they order human relations and influence the distribution of power. Technologies, Sclove instructs his readers, are "polypotent" products (p. 19), holding multiple meanings and capable of exerting latent social effects that may never have been in the minds of their original fashioners. As a result, the "focal function" (p. 21) of any technology—the function that it was ostensibly designed to serve—may be overshadowed by the gradual accretion of other, "nonfocal" functions that are deeply antidemocratic, as when nuclear weapons designed to deter totalitarian regimes begin to legitimate authoritarian social structures within democratic societies. The book's central premise is that a knowledgeable citizenry can choose technologies in ways that minimize such politically adverse externalities.

While these arguments are for the most part unexceptionable, the illustrative cases, chosen for didactic purposes rather than for novelty or depth, tend to romanticize the relationship between small-scale or decentralized technologies and democracy. The book opens, for instance, with an analysis of the installation of running water in Ibieca, a northern Spanish village, in the early 1970s. Sclove charges this apparently harmless development with having loosened important communal ties, particularly among women, who "stopped congregating at the washbasin to intermix their scrubbing with politically empowering gossip about men and village life" (p. 3). One suspects that Ibiecan women may not have viewed their daily collocations at the communal pump in quite so rosy a light. The example sets the reader wondering whether the author's gaze may not be reflective of very particular ethnic, historical, and cultural circumstances.

Suspicion deepens to certainty as the book progresses. Sclove's essentially Jeffersonian conception of democracy universalizes the values of grassroots activism, pluralist negotiation, and local self-sufficiency. The technologies he deplores are all those whose nonfocal effects take control out of the hands of individual workers, citizens, and communities, from assembly lines to nuclear weapons and from regional waste management systems to networks of electronic communication. By contrast, successful examples of democratizing technology include for him the largely white and middle-class achievements of the environmental movement in controlling pollution and of people with disabilities in winning barrier-free designs in the built environment. When Sclove looks outside America for lessons in the democratic steering of technology, his eye is drawn mostly to the gendered, industrial workplaces of northern Europe.

These choices betoken no lack of sympathy with the political aspirations of women, minorities, or citizens of the Third World. Sclove sincerely applauds the efforts of U.S. women's groups to change medical research designs and of the environmental justice movement to secure a more equitable distribution of technological risks. Yet, oddly enough in a book that takes seriously the sociological concept of "structuration" (p. 35), there is little acknowledgement of the obdurate sociopolitical structurings of race, class, gender, or capital. Sclove's breathtakingly ambitious strategy for achieving a more democratic politics of technology makes all courses of action sound equally plausible: from instituting "citizen sabbaticals" (p. 43) to changing the tenure criteria for academics working on technology and society; from increasing worker participation in R&D decisions to establishing more centers like the now-defunct U.S. Office of Technology Assessment; and from reinstating local currencies to imposing a "modest" (p. 229) tax on private R&D investments to fund citizen and expert collaborations in making technology policy.

Sclove is a man with a vision, and it is a vision that deserves attention if only because it is so radically at odds with that of today's captains of global industry. Much of what he advocates is humane, sensible, and—within limits—true to the findings of recent critical studies of technology. But in an intellectual landscape that has been remapped by works like Edward W. Said's *Culture and Imperialism* (1993), one has to ask whether a conception of democracy that seems wholly appealing in a prosperous New England academic community would travel equally well to the peasants of Chiapas or the pavement-dwellers of Bombay. One cannot help recalling that Thomas Jefferson, architect, humanist, and democrat of the Enlightenment, did not include slaves within his imagined community.

**Democracy, Education, and the Schools.** Edited by Roger Soder. San Francisco: Jossey-Bass, 1996. 293p. $29.95.

William A. Galston, *University of Maryland at College Park*

How do young people become good citizens of a democratic polity? What is the role of the public schools in effecting this outcome? These classic questions are addressed anew in the nine stimulating if uneven essays that this volume comprises.

The authors—feminist, ecological, strong-democratic, Straussian, among others—are not of one mind on much of anything. Still, one can discern a core and a periphery. The most-quoted and praised figures are Thomas Jefferson, John Dewey, and Benjamin Barber. The U.S. Constitution virtually disappears from view. Participation is preferred to representation. Liberalism is part of the problem. Nathan Tarcov questions the view that democracy is the sole political good and argues that "democracy has its own defects" (p. 33), but his is a lonely voice.

In her marvelous provocation, *American Citizenship* (1991), the late Judith Shklar urged a concrete, historically situated understanding of what it means to be a citizen in good standing of a modern commercial republic—not only the right to vote and participate in political debate, but also the capacity and opportunity to earn a living through gainful work. A few of the authors in this volume—notably Linda Darling-Hammond and Jacqueline Ancess—do address the role of the school in fostering economic opportunity; but many seem to view preparation for employment as somehow incompatible with the encouragement of citizenship understood in the traditional political sense. One wonders how this proposition would fare were it to be subjected to public deliberation—especially in this time of anxiety, when so many parents wonder whether their children can find even a tolerable place in the new economy.

More broadly, I am struck by the gap between the ostensibly democratic *proposals* advanced in this volume and anything resembling democratic *procedures* for assessing them. Robert Westbrook acknowledges that "participatory-republicanism is a minority and adversarial view to which few Americans are committed, or of which not many are even aware" (p. 140). But other authors do not confront the implications of this fact. For example, Walter C. Parker invokes John Dewey, but his preferred pedagogy for democracy is to be devised by "curriculum planners" who are responsible for specifying the aim of the entire enterprise. "Educators" may argue, but the people somehow disappear from the conversation about what and how their children should be taught.

It is instructive to see what happens when somebody bothers to consult the people about such matters. The Public Agenda Foundation did just that in 1994. They discovered that most Americans believe public schools are failing to provide the minimum prerequisites for learning—safety, discipline, and a focus on basic academic competences. In a context of safe schools and disciplined classrooms, the people support higher academic standards, testing, and accountability. Until these first-order goals are achieved, they are uncomfortable with education experts' focus on pedagogical innovations and "critical thinking skills." These sentiments do not define the limits of appropriate education reform or of education for democracy. But they represent voices that would-be reformers ignore at their political and moral peril.

Still, there is much to be learned from each of the essays in this volume. Nathan Tarcov reminds us of the range—and practical import—of philosophical understandings of democracy. Donna Kerr offers a complex personal vision of the relations between democracy, civil society, and nurturance of self and others. Mary Catherine Bateson sketches a democratic pedagogy in which the concept of ecology, broadly understood, takes center stage. John Goodlad puts forward a number of policy proposals, including a post-high school year of universal community service. Westbrook calls our attention to the burgeoning community school movement as one practical way of moving toward John Dewey's democratic vision. Darling-Hammond and Ancess show how unequal educational opportunities impede the formation of democratic as well as economic skills for those at the bottom of the socioeconomic ladder. Parker provides insights into the lengthy history of civic education curricula in American public education. Roger Soder produces a model list of texts for educating the teachers who will be responsible for instructing students in democratic theory and practice.

In my judgment, the single most compelling contribution to this volume is John Angus Campbell's discussion of the central role that the teaching of public speaking can play in democratic education. In Campbell's account, democracy requires acts of individual judgment within deliberative communities. Rhetoric, which requires the speaker to decide what to say about *this* topic before *this* audience on *this* occasion, shapes the capacity for judgment. If we want to produce persuasion or even induce reflection, we must look for some point of agreement with our audience; successful rhetoric stands on common ground and emphasizes the ability of community to bridge our differences. There are, Campbell concludes, "ethical norms intrinsic to the act of speech that usually or for the most part foster the development of a deliberating community capable of self-restraint" (p. 225).

If Campbell is right, as in the main I think he is, we could significantly advance the cause of democratic education by ensuring that all students have the opportunity to study the great public speeches of the past and learn for themselves how to frame persuasive arguments in the company of their fellow students. Here as elsewhere, the twenty-first century may have something to learn from the nineteenth.

**Perpetuating the Pork Barrel: Policy Subsystems and American Democracy.** By Robert M. Stein and Kenneth N. Bickers. Cambridge: Cambridge University Press, 1995. 232p. $49.95.

Gerald Strom, *University of Illinois at Chicago*

Consider a simple legislative system in which rational, elected legislators represent individual districts and want to remain in the legislature. Also assume that voters in these districts are influenced in their voting by the goods and services a legislator brings to his or her district. In such a system each legislator can be expected to exploit whatever legislative structures exist (e.g., committees) to try to supply as many goods and services to his or her district as possible (or at least enough to get reelected). Now, if we add a majority decision rule, one would expect that at least a majority of districts would share in the allocation of these goods and services. This, in brief, is distributive theory.

Now add an assumption that legislators are risk-averse about both their next election and their being members of the majority coalition that allocates the goods and services. Majorities in this case can be expected to be larger than minimum winning coalitions and may well be coalitions of the whole. As a consequence, many more than a majority of districts can expect to share in the distributions of governmental goods and services. This, in brief, is universalism.

Intellectually, both distributive theory and universalism are internally coherent and look appealing as explanations for the allocation of governmental spending. The problem, however, is that when one looks at actual federal programs, as Stein and Bickers do here, one finds that not even a majority of districts receive any monies (how many districts, for example, are eligible for federal tobacco price subsidies?). One might retort that it is not individual programs that are in the domain of these theories but implicitly or explicitly logrolled portfolios of programs (e.g., all federal agriculture programs). The problem is that not even these portfolios supply funds to a majority of districts. One might add a condition that universalism as a theory applies only to distributive policies that can potentially supply benefits to all districts, but this makes the domain of universalism virtually equivalent to the null set.

It would thus appear that neither distributive theory nor universalism is able to account for the empirical reality of how distributive policy allocations are made. Do we need new models? Stein and Bickers suggest not; instead, they suggest that the two extant models have neglected to include a key player in the policymaking process, namely, interest groups. In particular, they suggest that legislators can often be induced to vote for a program that does not provide goods and services to their districts by campaign contributions from interest groups. If legislators can enhance their reelection prospects by bringing dollars and programs to their districts, they can also enhance their reelection by building up a big campaign war chest.

Do interest groups operating in distributive policy areas distribute PAC contributions in such a way as to help build majority and supermajority coalitions for distributive policy? In the cases Stein and Bickers analyzed, it appears that they do. Not only do interest groups contribute to majorities and supermajorities of legislators, but these contributions appear to influence how legislators receiving them vote.

A final problem that Stein and Bickers address is the very weak relationship in the extant literature between success in bringing home the bacon and success at the polls. Their solution is to note that, among other things, the concern of legislators about the next election may be variable. Some might feel electorally secure and, as a consequence, not concern themselves as much with providing governmental goods and services to their constituents, while others may consider their next election to be highly problematic and invest a great deal of their resources in providing such goods and services to their constituents. Assuming legislators are reasonably accurate in their estimates, the empirical implication would be that some legislators win big without providing many goods and services, while others who do provide them only win close elections (or lose). As Jacobson found for campaign spending, this implies a possible negative statistical relationship between distributive benefits and electoral margins, even though such benefits are electorally beneficial. In this same regard, Stein and Bickers identify what could be called the paradox of pork: In legislatures like the Congress, those legislators most in need of supplying such benefits (e.g., freshmen legislators) are the least able to do so, while those with the seniority and other resources to supply them have less motivation to do so.

In conclusion, Stein and Bickers have significantly advanced the debate about distributive policy. This work establishes a baseline for all future work in this area, both in terms of the new ideas put forward (especially those concerning the role of interest groups in distributive policy decisions) and in the range of data that need to be considered in drawing conclusions about the allocation of distributive policy benefits.

**Post-Passage Politics: Bicameral Resolution in Congress.** By Stephen D. Van Beek. Pittsburgh: University of Pittsburgh Press, 1995. 227p. $49.95.

Walter J. Oleszek, *Congressional Research Service*

Citizens and commentators regularly talk about "the Congress" as if it were a unicameral rather than a bicameral body. Often forgotten is that the House and Senate are constitutionally, organizationally, procedurally, and culturally different and that these two lawmaking institutions play different roles in the policymaking process. As Alexander Hamilton wrote in *Federalist* No. 60, the Founding Fathers employed "dissimilar modes" of constituting the House and Senate to "permanently nourish different propensities and inclinations."

Despite their many differences (size, term of office, constituency, and more), the House and Senate must still work together if measures are to be enacted into law. Bicameral resolution of interchamber differences is the focus Stephen D. Van Beek's well-written and insightful study. More specifically, Van Beek concentrates on the role of conference committees in resolving bicameral disagreements and looks at how this is done given the rise of megabills hundreds of pages in length, multiple referrals and the massive conference delegations spawned by them, the increase in the number of overlapping and competing provisions reported by the panels that receive a multireferred measure, and other congressional developments of the past few decades.

A legislative assistant to former Rep. Tony Coelho, D-Calif., in the mid-1980s, Professor Van Beek ably draws upon his personal experiences, congressional contacts, and scholarly analyses to stretch our understanding of how conference committees actually conduct their business in the new legislative environment of greater participation, openness, and complexity. Using a case study approach with bills drawn from the 100th and 101st Congresses—the savings and loan bailout, an omnibus trade act, and a budget reconciliation measure—Van Beek examines the processes employed on disparate legislation to achieve bicameral resolution and the roles and strategies employed by individual conferees to reach this goal. The case examples are deliberately selected to illuminate and contrast how bicameral decision making proceeds when it is largely dominated by committees (the savings and loan bill) and when numerous participants have a hand in shaping the conference product (omnibus trade and budget reconciliation).

Van Beek's work is more than a case study treatment of three complex and controversial megabills. Significantly, what the author successfully accomplishes is to demonstrate that today's conferences on omnibus measures involve a broad array of actors and influences and reflect a legislative process different in many respects from traditional views of how our laws are made. The "old" conference style, where managers from a single committee in each chamber haggle over interchamber differences to reach an accord, no longer suffices as the only model for conceptualizing the process of reaching bicameral compromises. Like so many things in the House or Senate, such as the greater variety of "rules" from the House Rules Committee or the new intricacies associated with Senate unanimous consent agreements, procedures and processes have gotten more complicated on Capitol Hill. Needless to say, the same development has occurred with "the third house of Congress," as Senator George Norris of Nebraska once called conference committees.

Many of today's conferences, because of the "scope and saliency" of the issues requiring interchamber resolution, "are only one part of the larger process of bicameral resolution" (p. 5). The larger process involves both conference and nonconference actions. As Van Beek explains, "Post-passage politics refers to the fact that the politics of resolution are part of a continuous process that begins as early as when the first chamber is passing its bill and before the second chamber has acted. In these instances members, anticipating the onset of a conference, adjust their behavior in an effort to improve their conference bargaining position" (p. 5).

Nonconference actions involve more than lawmakers in each chamber planning their preconference moves (adding amendments as bargaining material, for instance) in anticipation of this negotiating stage. It means, too, that on some megabills the real decisions on the matters in bicameral disagreement are made by parties other than the formally designated House and Senate conferees. Congressional leaders and the president, for instance, hammer out accords in summits or other private bargaining sessions. These agreements are then formally implemented by the conferees. Typically, party leaders and executive officials monitor and coordinate conference activity to resolve logjams and to promote bicameral agreements.

In sum, Van Beek has produced a solid and readable account of conference policymaking and contributed to our understanding of this penultimate, yet often little-examined phase of lawmaking. Although his case studies are short and the contribution to theory limited, Van Beek's book points the way to further research. He challenges arguments put forth by Congress-scholars that conference committees are creatures of committees or their parent chambers; instead, Van Beek asks that scholars pay more attention to the increasing complexity of omnibus legislation and how this development influences who participates in and affects the dynamics and outcomes of conference bargaining.

Professor Van Beek also revisits and raises important questions, such as should conferees have discretion to make policy on issues that were never considered by their parent chamber; are deliberation and accountability hampered by the new style of resolving bicameral disagreements on megabills; are conference committees the prime policy integrators on Capitol Hill; and what can be done to address the large size of many contemporary conferences, which typically lengthens the reconciling process because so many interests clamor to participate in what are often interchamber and interbranch negotiations? In short, *Post-Passage Politics* should stimulate further research into the role and performance of conference committees.

**Communication in Congress: Members, Staff, and the Search for Information.** By David Whiteman. Lawrence: University Press of Kansas, 1995. 252p. $35.00 cloth, $15.95 paper.

Bruce Bimber, *University of California, Santa Barbara*

In *Communication in Congress*, David Whiteman takes on a question that has lingered in the background of Congress studies for two decades: What is the role of policy analysis in the legislative process? For the most part, conclusions drawn by Nelson Polsby, Charles Jones, Allen Schick, and other scholars during the 1970s have remained the accepted wisdom on the subject ever since: Congress is informationally challenged. The structure of legislators' incentives means that Congress is unable to make systematic use of the tide of expert information directed at it. As Jones put it in a 1976 article, Congress "can't do policy analysis."

Whiteman argues convincingly that political scientists have been too focused on the absence of connections between policy studies and members' voting decisions to see that the real action lies in communications networks that link legislators' offices with experts inside and outside Congress. These networks can play a vital role in connecting policy experts with legislative staff long before legislators themselves record their votes on bills. Moreover, Whiteman claims, traditional studies of information in Congress have failed to differentiate between what goes on inside the core of issue networks and what takes place further out at the periphery. For that often-small group of legislators operating in the core of a particular issue network, Whiteman argues, expert information can play a significant role. Whiteman's book offers an answer to an obvious empirical puzzle posed by the conventional wisdom: If Congress is at best indifferent and at worst hostile to policy analysis, why has it invested so many resources in staff capacity to analyze policy? The answer suggested by Whiteman's book is that while most legislators may be disinclined to consult the claims of experts on any particular issue, there is typically a core of highly involved legislators whose offices draw regularly on experts. He elaborates a number of variations on this basic pattern, and the result is one of the most richly detailed examinations of the subject available.

Whiteman labels his approach "network analysis," and the foundation of the book is a detailed description of who communicates with whom under what circumstances in Congress. He builds upon an observation made by other scholars, namely that Congress can be understood as a collection of enterprises, each headed by an elected member and operated by a group of staff with diverse skills, talents, and interrelationships. Whiteman insists that we abandon the familiar conceptualization of Congress as 535 individualistic decision makers and accept the idea that the institution is a collection of small organizations, each embedded in various issue networks. It may take several chapters before some readers are accustomed to Whiteman's use of the term "enterprise" where one is used to seeing "legislator" or "member." For instance, Whiteman describes legislators' positions in Congress with phrases such as "enterprises that occupy leadership positions" or "enterprises on the same committee" (p. 44) in order to drive home his point.

Whiteman examines four issue networks in the 99th Congress, and the book is organized around a set of descriptive typologies drawn from these basic case studies. The typologies categorize types of congressional enterprise, sources of policy information, modes of communication, forms of information search, ways the legislators use policy analysis, and so forth. For instance, Whiteman describes the varying structure of legislators' offices in a fivefold typology—collegial, corporate, folk, formal, and unitary. Much of the empirical consequence of the book is established by Whiteman's extensive use of interview respondents' own words. On some pages, interview quotations are a significant part of the narrative.

Ultimately, Whiteman is interested in showing how patterns of communication and information acquisition inside these enterprises vary, most importantly as enterprises shift from peripheral to more central roles in policymaking. He finds that where legislators and their staffs are operating at the core of an issue network, staff are more autonomous and communicate more heavily with policy experts. He uses regression analyses on his interview findings to address an issue in the literature on knowledge utilization: correlations between variables describing the context of policy problems and the ways in which legislators use information. Among his findings is the claim that the influence of policy experts declines as the salience of an issue for a legislator's constituency increases.

Along the way, Whiteman provides a variety of interesting glimpses into the routine workings of the legislature: how issues get on the agenda, how staff personalities can shape communication and information gathering, problems caused by staff turnover, and so on. Readers interested in a richly textured narrative about the details of congressional life, especially that of staff, will find it here, although those with less interest in the nitty-gritty of who talks to whom in Congress may find too much detail.

Although it is not Whiteman's mission to attack rational choice theorizing, one cannot avoid noticing that his findings support critiques of the veracity of assumptions in formal models of the legislature. Until relatively recently, legislators were assumed to possess complete and perfect information in legislative "games," while in more recent models involving asymmetric information, legislators are assumed to make decisions to acquire and reveal information under well-specified conditions of strategic calculation.

Many political scientists need no further evidence that assumptions in this modeling tradition are sometimes wildly unrealistic. But Whiteman's case studies and tabulations of types of information searches represent another claim for just how far from the real Congress such models lie. Whiteman's portrait of legislators' "enterprise context" is more consistent with the organizational models of James March and his collaborators than with the traditional incarnations of the rationality model.

This all adds up to an account whose 300 or so interviews do a solid job of supporting the case studies, but the age and scope of the evidence give one pause. The actions Whiteman describes took place a decade ago. He does not provide evidence from more recent years to deflect obvious questions about what has changed since the 99th Congress squared off against the Reagan administration. None of the issue networks described here involve major, high-profile legislative conflicts. Whiteman offers the reasonable argument that the cases are representative of the kind of routine legislative work in which Congress is regularly involved, but one wonders how different the findings might look in the case of big-ticket legislation and what those differences might tell us about the larger communication picture. These reservations notwithstanding, Whiteman's description of legislative communication does ring true.

Those readers looking for a new theory of communication in Congress are likely to wind up wishing that Whiteman had gone farther in the direction of theory building. But his typologies and his identification of patterns in communication move the study of policy analysis information in Congress a step forward, and they constitute a credible claim that Congress can "do policy analysis" after all.

**The Myth of Democratic Failure: Why Political Institutions Are Efficient.** By Donald Wittman. Chicago: University of Chicago Press, 1995. 240p. $29.95.

Barry R. Weingast, *Stanford University*

On the whole, economists are known in political science for their pessimistic views about democratic politics and the performance of public programs. Nearly all political scientists are familiar with economic analysis of particular topics, including interest group bias, collective action problems, rent-seeking, political business cycles, government or political market failure, inefficient bureaucracy, and ideological shirking by elected representatives. Taken as a group, these concepts reflect a remarkably cynical view of the democratic process.

Working counter to this trend is Donald Wittman's *The Myth of Democratic Failure.* Wittman, an economist at the University of California at Santa Cruz, presents a remarkably sanguine view about democracy. As his title suggests, Wittman argues that, despite its portrayal in the literature by economists and rational choice political scientists as the seemingly dominant view, democratic failure is a myth.

At a broad level, I agree with Wittman; economists' arguments remain fundamentally incomplete, if not entirely wrong. If, as the economists emphasize, politics systematically replaces efficient markets with inefficient government programs, how did thriving markets ever arise in the first place? Economists' theories fail to explain how the United States grew so rich in the two centuries following 1787.

If governments sometimes encroach on markets, this is not their sole activity. Indeed, as economist Douglass North argues, governments are also intimately connected with thriving economies. Existing theories—whether developed by economists or political scientists—tell us little about how governments interact with private agents to promote economic growth.

Although economists often deride "democratic political markets" relative to economic ones, Wittman argues that political markets are not less efficient than economic markets. The reason is that political competition in democracies is just as effective as economic competition in markets. Politicians and bureaucrats who pursue their own self-interests are no more a drag on political efficiency than are businessmen who pursue their own self-interests (p. 114). Put another way, self-interested behavior in the face of competition provides social benefits in both politics and economics.

Wittman organizes his book with an Introduction followed by thirteen chapters divided into three parts. In Part I, "The Factors for Efficiency in Political Markets," Wittman argues that "behind every model of government failure is an assumption that the voters are poorly informed, serious competition is lacking, and/or transactions costs are excessive" (p. 192). The chapters of Part I are designed to show that, despite their common acceptance, the economists' assumptions are poor ones. In chapter 2, Wittman argues that voters are more informed and make better decisions than was previously believed. The following chapter seeks to demonstrate how "electoral-market" competition helps limit opportunistic behavior by public officials. Chapter 4 focuses on transactions costs and the design of government institutions. Wittman argues, for example, that the small size of Congress allows members to create institutions and practices that support efficient decisions. Chapter 5 rejects recent work in cognitive psychology, particularly Kahneman and Tversky's "prospect theory," as a means of explaining democratic politics.

Part II, "The Organization of Political Markets," turns to the analysis of the choices made in the political arena. Chapter 6 studies "Legislative Markets." Wittman's arguments contrast with much of the work in the 1980s on Congress, which focused on members and committees. Echoing more recent work of the 1990s, Wittman argues that parties and the collective organization of each chamber constrains the behavior of members and committees, mitigating or even eliminating inefficient decisions such as pork barrel expenditures.

Chapter 8 turns to "Bureaucratic Markets" arguing (unsurprisingly) that they are neither inefficient nor too large. Paralleling the ideas of Fiorina, McCubbins, and Moe, this chapter argues that, because politicians control the design of delegation and the degree of competition among bureaucracies, they also control the degree of bureaucratic inefficiency. Wittman suggests that any inefficiencies remaining in the bureaucracy are by design. But since, by the argument of chapter 6, politicians want efficient ends, they will design structures of delegation that foster those ends.

Chapter 7 challenges much of the standard litany about interest groups. Chapter 9 studies "The Market for Regulation," while chapter 10 examines "The Constitution as an Optimal Social Contract." The book ends with a short Part III focusing on methodological concerns, such as standards of proof.

Many will find this book unsatisfying, in part because countering several decades of analysis is a monumental task. Wittman attempts to refute dozens, perhaps hundreds, of arguments about inefficient government, and many are given insufficient attention to develop a convincing counterargument. As a consequence, the book appears a bit more Pollyannaish than it should be. A more focused book, with more evidence and in-depth case analysis, would have helped—although such a book's smaller breadth may have made it less provocative.

Will most of Wittman's assertions about political efficiency stand the test of time? Perhaps not. However, the value of this book is not so much its provocative conclusions, but that it engages us to rethink old assumptions and approaches. Wittman's arguments are refreshing, and his book provides a new voice in the debate about the economic effects of democracy.

**Abortion Politics in the Federal Courts: Right versus Right.** By Barbara M. Yarnold. Westport, CT: Praeger, 1995. 156p. $49.95.

Laura R. Woliver, *University of South Carolina–Columbia*

Examining the decisions, litigants, and interest groups involved as amicus curiae in 145 federal district court cases from 1973 to 1990 that cite to *Roe* v. *Wade* (1973), Barbara M. Yarnold concludes that pro-life interests are at a disadvantage in the federal courts and that pro-choice interests are favored. The analysis displays once again the political nature of the federal courts, and details which regions of the country, which type of litigants (repeat players or not), and what kind of judicial constituency, among other variables, are more or less successful with federal abortion litigation. Yarnold discusses the pros and cons of coalitions of groups in cases and the multiple-clinic nature of some litigation. She also discusses the mission statements of some interest groups and associations involved in abortion lawsuits and points out how their abortion politics activism is not clearly acknowledged in their written communications to members.

Two questions raised by this study regard how to measure and how to count abortion cases. Yarnold categorizes each abortion case as either a win or a loss for the pro-choice side. Yet, there are many important cases where both sides have won a little and lost a little. *Roe*, for instance, has been weakened over time by numerous decisions that uphold it, rule unconstitutional some aspects of a state abortion reform act, and accept other parts of the same act. To claim, therefore, that pro-life interests are disadvantaged in the federal courts because of a dichotomous emphasis on "winning" or "losing" is to mismeasure the impact of pro-life and pro-choice interests in the federal courts.

A related issue is whether interest group staffers, especially in a highly contested area such as abortion politics, tell researchers the whole story in response to mailed surveys or telephone interviews. Yarnold acknowledges difficulties in obtaining financial information from clinics and others involved in abortion politics. Nevertheless, she accepts the self-reports of interest group staff concerning their assets and their activities. Yet, interest group assets can be underreported, because groups have affiliated groups or PACs that they might not incorporate into answers to questions about resources. In-kind contributions and soft money also would not necessarily be reported to researchers. One example of how such underreporting might play out is Yarnold's finding that Americans United for Life had a 1992 operating budget of $250,000 and a staff of twenty (p. 110). This doesn't seem to add up. Rent on their offices in Chicago, the bills for their phone lines, and staff salaries would probably far exceed $250,000. As groups tend to be quite cagey about revealing their affiliations and resources to scholars, Yarnold might have dug deeper to ascertain a fuller picture of resources and activities.

The author's research was also hampered by the reluctance of leaders of abortion interest groups and allied associations to respond to her initial survey or provide other information. Yarnold is to be commended for her perseverence through seven months of follow-up telephone calls to these group leaders, who are often not very helpful to abortion politics scholars. Through her efforts, Yarnold was eventually able to secure a 76% response rate in her survey of 166 organizations (from an initially low response rate of 7% in the first survey). In her survey, however, respondents were left to themselves to define what some key terms might mean. According to the survey instrument reproduced in one of the appendixes to the book, guidelines clarifying what the researcher would categorize as "frequent" or "infrequent" contacts with government officials were not provided. One group, then, might respond that they *frequently* lobbied Congress over the appointment of federal judges based on two dozen lobby contracts, while another group who also lobbied two dozen times might respond that they *infrequently* contacted congresspeople.

In addition to her discussion of measurement issues, Yarnold engages in some troubling causality assumptions. In two places in the book (pp. 43–4 and pp. 92–3) Yarnold draws on an assertion made by Bill Baird in an 1991 interview with her concerning the business practices of the Planned Parenthood Federation of America. Yarnold does not verify these claims by Baird, nor does she check out her source. Baird is characterized in David Garrow's *Liberty and Sexuality* (1994), for instance, as a long-term critic of Planned Parenthood. This does not mean that Baird's assertions are therefore untrue, but Yarnold relies exclusively on his statements without further corroboration or independent confirmation. Yarnold twice points out (p. 44 and p. 93) that many of the initial surveys she mailed out to private abortion clinics were returned because the clinics had been dissolved. She employs this result as evidence of Baird's claim that Planned Parenthood was driving competitors out of business. The causality assumptions here are troubling. There are, no doubt, many reasons why clinics might close; after all, a large percentage of small businesses of all types close every year. Pro-life pressures have closed many clinics, too. Yarnold does not examine the complexity of why clinics close, nor does she draw on the published research on clinic access; rather, she implies a linkage to the claims of one interviewee.

The literature relied on in this book is constricted. Several major studies in abortion politics are not mentioned at all, while many others can be found only in long lists of citations. This work would have benefited from the incorporation of additional research by the author.

Despite such troubling issues of measurement, causality, and use of the literature, this study is a step toward examining the various factors that might be affecting federal court decisions in abortion cases. Yarnold is right about both the contentiousness of abortion politics and the need for further research on many aspects of this important topic.

## Comparative Politics

**Value Change in Global Perspective.** By Paul R. Abramson and Ronald Inglehart. Ann Arbor: Michigan University Press, 1995. 180p. $44.95 cloth, $16.95 paper.

Raymond M. Duch, *University of Houston*

Over the last three decades, Ron Inglehart's theory of Postmaterialism has had a major influence on the study of comparative politics. Hence, *Value Change in Global Perspective* by Paul R. Abramson and Inglehart deserves the serious attention of students of comparative politics, particularly scholars interested in the issue of political culture. The book accomplishes three goals: There is a brief restatement of the Postmaterialist argument, particularly as it relates to changing Postmaterialist values; it provides further evidence supporting the thesis; and finally it contests, once again with data collected by the authors, recent empirical challenges to Postmaterialism. This book is a must-read for scholars of political culture, but it is also an interesting teaching tool because of the manner in which it encapsulates an interesting theoretical and empirical debate in the field of comparative politics.

The core element of Postmaterialist theory is that economic security created by advanced industrial societies gradually changes the goal orientations of the mass public. Those growing up during periods of relative economic security are Postmaterialists. The authors describe most Materialist values as "universal but often latent needs for belonging, esteem, and the realization of individual intellectual potential." Postmaterialists place an emphasis on the need for freedom, self-expression, and improving the quality of life. Those maturing during periods of scarcity retain Materialist values that emphasize economic and security needs. One of the goals of this book is to evaluate the important implication of this thesis. Specifically, Abramson and Inglehart argue that because of the differences in the formative socialization experience of post–World War II cohorts, younger cohorts, particularly those in Western Europe, are increasingly Postmaterialist and that there has been a significant shift toward new value priorities in the advanced democracies because of intergenerational population replacement. A second goal of the book is to challenge two recent critics of Postmaterialism.

The book is nicely organized around major controversies associated with the Postmaterialist thesis. It begins with a chapter restating the argument and presenting evidence that there is a decided trend in values in the advanced democracies toward increasing levels of Postmaterialism. A subsequent chapter distinguishes between short-term and long-term trends in Postmaterialism. The authors note, as many others have observed, that the measure is highly sensitive to fluctuations in inflation levels. Most important, this chapter addresses the argument leveled by Harold Clarke and Nitish Dutt ("Measuring Value Change in Western Industrial Societies, *American Political Science Review*, 1991, pp. 905–20) that the measure is flawed because "concern with levels of unemployment," which most would consider to be a materialist value, is positively correlated with the Postmaterialist construct. This is followed by a chapter that addresses the Raymond Duch and Michael Taylor ("Postmaterialism and the Economic Condition," *American Political Science Review*, 1993, pp 747–79, and "Education, Security, and Postmaterialism," *American Journal of Political Science*, 1994, pp. 815–24) critique that challenges the notion that economic conditions at the period of early socialization actually result in Postmaterialist values. They argue that education is the primary variable predicting levels of Postmaterialism and conclude that the Postmaterialist construct might better be considered a measure of democratic values. In both of these chapters Abramson and Inglehart challenge a number of the empirical findings on which these authors build their critique of Postmaterialism. Finally, the authors end with an evaluation of trends in the development of Postmaterialism in developing nations.

The book represents a spirited defense of the authors' thesis regarding postmaterialism. The real strength of the book is its documentation of the trend in Postmaterialist values and projecting future levels. But in some respects this is the least controversial aspect of the authors' work on this concept. Both major critiques of the work (Clarke and Dutt; Duch and Taylor) would not find fault with the authors' argument regarding generational replacement. Another strength of the book is that the authors present the positions of their critics in a very fair manner. Hence, readers, and particularly students, have an opportunity here to view some of the more serious criticisms of the Postmaterialist thesis.

Having said this, a number of weaknesses of the book need to be noted. First, the authors are confronted with somewhat of a dilemma. Clearly, the book is written for a broad audience of political science scholars and students. Hence, the manner in which the authors address the major critiques of their work is not as detailed and rigorous as I would prefer. Given the audience of the book, this is a reasonable strategy, but the authors should have been more circumspect with respect to the conclusions they draw. The evidence they present in the book does not definitively undermine the major critiques (moreover, in some cases the authors suggest some dubious interpretations of the tabular data that they present). I believe it would take a different kind of book and a different level of analytic rigor to accomplish this goal.

The theory of Postmaterialism is compelling and widely accepted. It is, at the same time, a theory of how preferences are formed; the role economic, social, and political factors play in shaping these values; how these values change over time and the factors that are responsible for these changes; and how these values shape political behavior and the political implications of these correlations. This wide-ranging scope of the theory is responsible for one of the more general problems I see with this book. The authors do not focus on a rigorous analysis of any particular aspect of the theory but rather opt for addressing a rather wide range of issues in a relatively superficial manner. At this point in the history of Postmaterialism, students of the concept are interested in a much more intensive effort to establish the fragility of the theory, exploring more intensively, for example, how it stands up to competing theories of value formation and value change. Hence, this book will leave those scholars with a keen interest in Postmaterialism somewhat unfulfilled and anxious for more.

**Towards Illiberal Democracy in Pacific Asia.** By Daniel A. Bell, David Brown, Kanishka Jayasuriya, and David M. Jones. New York: St. Martin's Press, 1995. 197p. $59.95.

Ross Marlay, *Arkansas State University*

Daniel Bell and his colleagues have put together a valuable contribution to the scholarly debate over Asian democracy, but it will not be the last word on the matter. Surprisingly, for a book containing seven chapters by four authors, it holds together well and presents a coherent viewpoint, which can be summed up as follows: Asians do not understand democracy in the same way as Westerners, for they have "alternative cultural baggage." They do not seek equality because they have a hierarchical worldview. Nor do they want freedom, if by that is meant individual autonomy, for they prefer "familism." Asian countries that have conducted democratic elections have done so to promote stability, not freedom. Western liberals may hope that economic modernization will create clashing interests, which in turn will foster democracy, but the overwhelming dominance of state power in Asian nations will prevent that. One author (Kanishka Jayasuriya) seems to go even farther. He denies that democratization in Pacific Asia has anything to do with the empowerment of civil society; it merely "reflects a renegotiation of the relationship between political elites and capital" (p. 108).

From the point of view of a Western liberal (in the modern or classical sense of that word) this book makes depressing reading. Most Americans assume that economic progress of poor countries will create a middle class, that necessary foundation of a democratic state. But these authors do not think much of middle-class Asians: They are "anxiety ridden" (p. 80) and exhibit "a corporatist or clientelist mentality" (p. 92). They are "anxious" and "uncertain," and their middle-class culture is characterized by "an absence of critical awareness, creativity, and self-confidence" (p. 93). Middle-class businessmen on Taiwan are "politically immature," and in South Korea they are "schizophrenic." Singaporean middle-class people are so "neurotic" that they sustain a "neurocracy" (p. 95). Goodness gracious!

Most of this book is not so extreme. Chapter 3 ("Democracy and Identity: The Paradoxical Character of Political Development" by David M. Jones) contains a learned interpretation of Confucianism and neo-Confucianism and an equally illuminating appraisal of political syncretism in Southeast Asia. Professor Jones's writing style, however, is marred by an unfortunate weakness for sentence fragments without verbs (two are found on page 67 alone).

Bell and his co-authors make the point that Western democracy evolved out of a set of specific economic, cultural, and social circumstances that simply do not obtain in Asia. We can grant that point without accepting their contention that Asians are uninterested in freedom or their conclusion that for the foreseeable future Asian countries will at best be "illiberal" democracies. They seem willing to call a country democratic if it holds reasonably fair elections, even if it is radically unfree in other ways. In their concluding chapter, the four authors specify three distinctive features of East Asian illiberal democracies: "first, a non-neutral understanding of the state; second, the evolution of a rationalistic and legalistic technocracy that manages the developing state as a corporate enterprise; finally, the development of a managed rather than a critical public space and civil society" (p. 163). Readers may begin to wonder just what the distinction is between an "illiberal democracy" and a repressive regime.

Some things are missing here. "Pacific Asia" seems to include South Korea, Taiwan, Singapore, Malaysia, Indonesia, and sometimes Thailand. These countries are illiberal, it is true, but what about Japan? A half century of democracy there is ignored. What about the Philippines? There is not a single word anywhere in this book about the bravery of tens of thousands of Filipinos who blocked a dictator's tanks with their bodies in 1986. What about Aung San Suu Kyi's followers? What about the students and workers of Tiananmen who *died* for democracy? They don't rate a mention either. A more balanced picture would emerge if the authors considered countries that did not fit their paradigm.

The matter of generational change also should have been considered. Even if older Asians were indifferent to freedom, as Bell et al. suppose, those in their twenties and teens have grown up with very different influences. Are we supposed to think that the return of all those young Koreans and Taiwanese who took their studies in the United States and Australia will have no impact on their societies?

The book is thoroughly indexed and contains an extensive and valuable bibliography.

**Fragile Democracies: The Legacies of Authoritarian Rule.** By Gretchen Casper. Pittsburgh: University of Pittsburgh Press, 1995. 202p. $49.95 cloth, $19.95 paper.

Belinda A. Aquino, *University of Hawaii at Manoa*

Using a number of cases in Latin America and the Philippines as recent exemplars, this book seeks to demonstrate that the aftermath of authoritarian rule is usually a fragile democracy. The principal argument advanced in the book is that authoritarian regimes seek to alter or transform the traditional roles of important social institutions, which continue to intervene in politics even after such regimes are gone. Thus, the transition to democracy "remains problematic at best," even in such countries as the Philippines that have "a strong democratic tradition."

The author analyzes the roles played by the church and the military in ousting the Marcos dictatorship in the 1986 "people power revolution" in EDSA (Epifanio de los Santos Avenue), Metro Manila's major highway. The EDSA Revolution, as it is popularly known, was triggered by a military mutiny against Marcos led by then Defense Minister Juan Ponce Enrile and top police general Fidel Ramos. Jaime Cardinal Sin, the Catholic church's highest leader, supported the Enrile-Ramos uprising and issued a call to the people to go to EDSA. Hundreds of thousands of the faithful turned up and formed human barricades against the regime's advancing tanks. Within three days Marcos was forced out of office and went into exile in Hawaii after the U.S. government made the necessary arrangements.

The author conducted open-ended interviews with a number of church and military leaders regarding their political role in the EDSA upheaval. Her theoretical framework is derived from the literature on democratic transitions in Latin American countries following periods of authoritarian rule.

The book's strong point is its comprehensive analysis of social institutions as political actors in times of national upheaval and what happens to them as things settle down. Although EDSA has been overanalyzed, the author's careful analysis of the role of the church and the military in toppling Marcos gives those not familiar with this particular case a better understanding of the political forces that have led to dramatic changes in Philippine society. The author also reflects in great detail on the dynamics of conflict and change within the church and military ranks, which led to such major developments as the politicized clergy and the reform movement in the military that were critical in challenging Marcos's authoritarian rule.

There are weaknesses in the book as well, which are essentially due to the limitations of the author's theoretical perspective on the fragility of democratic regimes following their authoritarian antecedents. What would have been helpful is a thorough discussion of the nature of Philippine democracy. While it is true that Marcos systematically undermined or destroyed established political institutions during his dictatorship, his work was made easier by the fact that democracy, through the many years of U.S. colonial rule and Philippine postcolonial administrations, was already fragile. The author's lack of critical analysis of the U.S. role in shaping a weak Philippine democracy is a major flaw of the book. American colonial policy installed a political infrastructure for democracy in terms of "organic acts" designed to prepare the islands for self-government, a strong presidency, elections, political parties, political institutions like legislatures, judicial systems and local governments, and concepts of due process and popular representation. But these political forms did not have a strong foundation of democratic values, such as equality and social justice, because U.S. policy did not encourage economic redistribution and structural changes, such as land reform, in a country where feudal values continued to prevail. American colonial rule nurtured and used the Philippine elite to advance their mutual interests, rather than attempting a meaningful reorientation of Filipino society. The historical insurgencies of Muslim groups, communist rebels, and other disaffected elements in Philippine society persisted for extended periods, proof of the fragility of any "democratic traditions" that have developed in the country's contemporary history.

Ironically, for all its activism during the Marcos regime, the institutional church has contributed to the nonviability of Philippine democracy by its regressive leadership and failure to address fundamental issues of social and economic inequality. Only the worst excesses of the Marcos regime goaded the church to action. Now that democracy has been restored, the progressive posture of church leaders that was evident when Marcos was around is nowhere to be seen. This raises the question of whether the church has to wait for a crisis as severe as EDSA again before getting involved in basic issues in Philippine society.

Similarly, the military restiveness of the Marcos years appears to have been contained since 1989, in part because President Ramos co-opted many of his former military colleagues in his administration. Thus, we may question the book's contention that "after the installation of democracy [in the Philippines], neither the church nor the military returned to their pre-martial law roles" (p. 179). The situation is not that simple.

In fact, the sentiment of most Filipinos is that the 1986 "revolution" was essentially a restoration of the pre-martial law status quo. Some changes have occurred in the party system, the economy, and the expansion of the people's and nongovernment organizations, but the seeming ideological change in church and military ranks during the Marcos regime appears to have waned. In the case of the military in particular, it used to be said that once the genie was out of the bottle, it would not be easy to put it back in. The military rebel leader Gringo Honasan, who was given amnesty, decided to run for a seat in the Philippine Senate and won, presumably with the support of President Ramos, Honasan's former military superior. Ramos has also appointed many of his former colleagues in the military to civilian positions. There are observations that a "silent coup" may already have occurred in Ramos's government. In effect, the politicized role of the military, which peaked during the Marcos regime, has been preempted by Ramos.

Meanwhile, Jaime Cardinal Sin is again pursuing his favorite issue: the "sinfulness" of family planning. One would think he would be concerned more with social issues after the country's experience with Marcos, but he does not seem to be moving forward with the times. In the 1995 senatorial elections, he and his leading bishops campaigned secretly against then Health Secretary Juan Flavier, opposing the latter's advocacy of birth control methods and family planning. The church's current political role is to attempt to block or blunt the Ramos family planning initiatives.

Another weakness in the book is its failure to distinguish between the Aquino and Ramos governments in terms of their relationships with the church and the military in the post-Marcos period. The military attempted to seize power from Aquino because they thought she was too soft on the Communists and they did not trust her ability to govern. Whether this distrust was in part due to gender issues is worth exploring. With Ramos, the military is more at ease for understandable reasons: He is one of them.

The church was fully supportive of Aquino, a devout Roman Catholic. Church leaders were at best only mildly critical of the corruption that Aquino's relatives were reported to be involved in. With Ramos, the first Protestant president of the country, the church has been disturbed and severely critical, particularly on the issue of population control.

These are some differential developments or details in postauthoritarian regimes that cannot be adequately explained by the conceptual framework used by the author in the Philippine case. Nevertheless, the book is an important contribution to the growing literature on redemocratization in Third World countries.

**The Workers' Movement in Russia.** By Simon Clarke, Peter Fairbrother, and Vadim Borisov. Brookfield, VT: Edward Elgar, 1995. 431p. $79.95.

**Soviet Workers and the Collapse of Perestroika: The Soviet Labour Process and Gorbachev's Reforms, 1985–1991.** By Donald Filtzer. Cambridge: Cambridge University Press, 1994. 295p. $54.95.

Darrell Slider, *University of South Florida*

Worker unrest was one of the many factors contributing to the collapse of Gorbachev's reforms and ultimately the Soviet Union itself. The number and intensity of strikes increased substantially in 1989 and 1990, and the political nature of many of the workers' demands paralleled at least superficially the activities of the Solidarity movement in Poland.

The role of workers and the rise of independent labor movements is the focus of both books under review. In spite of the common theme, however, they are complementary rather than overlapping. Filtzer's book is the more systematic survey, examining the overall role of workers in the former Soviet system. (The book is the third of a trilogy by the author on the history of the role of Soviet workers.) Clarke, Fairbrother, and Borisov examine the rise of independent trade union movements in the Gorbachev and early Yeltsin periods.

Filtzer explores the changes brought about by *perestroika* in the Soviet labor market and in enterprise behavior. He argues that traditional forms of shopfloor resistance made enterprises unmanageable at a time when Gorbachev's reforms were pushing them toward the market. In this analysis, the author tends to exaggerate the extent to which market-based reforms were a part of the Gorbachev program. Even under Yeltsin's "shock therapy" and the massive privatization campaign, many elements of enterprise governance remained unchanged, including the Soviet-era proclivity toward overstaffing and the easy availability of state subsidies and credit. Unemployment and enterprise bankruptcy were not widespread even after three years of Yeltsin's "radical" reforms. Workers were largely protected from the impact of reforms by paternalistic managers and government policy that discouraged factory closings and significant layoffs.

Filtzer contributes little that is new to the discussion of the politics of labor policy; instead of seeking explanations from a broader examination of *perestroika* and its sources, he hypothesizes from a neo-Marxist model of the relationship between the Soviet working class confronted by "a full-scale restoration of capitalism," thus opening the possibility of "the reemergence of class struggle" (p. 223) both in the former Soviet Union and in the West. Filtzer's resort to this approach to give meaning to the broader context is in part a function of the sources he uses, which are mostly limited to discussions of the problems of implementation of the regime's labor policy. There is little here that would help us understand the Gorbachev and Yeltsin regimes' strategic choices in economic policy.

Clarke, Fairbrother, and Borisov provide three extended case studies of independent worker organizations. These are based on in-depth (and at times excessively detailed) descriptions of the most significant new institutional actors in the labor movement.

The first case study, one that comprises about half the book, is that of the coal miners in the Kuzbass region. Because of the dependence, particularly in winter, of the Soviet and Russian economy on coal, miners were in a position to place serious economic pressure on the central government authorities. This made it possible for union leaders to expand the scope of their demands beyond wages and employment issues to issues of national politics. The interests of the miners were soon torn, however, between support for overcoming the communist system and the difficulties—particularly acute in the coal sector—caused by Yeltsin's economic reforms.

The second case focuses on several of the small number of enterprises where the trade union Sotsprof emerged as an important actor. The authors conclude that Sotsprof's influence was extremely limited (and fading by 1993–94) and that the organization was actually less a trade union than a vehicle for political activities by its core leadership. Even at individual enterprises, the strength of Sotsprof often depended on its ability to play on divisions among the competing groups of would-be owners.

The third case represents what the authors consider to be perhaps the most successful of the independent trade unions, one which joined Russia's air traffic controllers. It differed from the other movements discussed in that it was a outgrowth of an official Soviet-era trade union. Strikes in the post-Soviet period were followed by repressive government measures, but the union was able to use its contacts and a fortuitous change in the organization of the air control system to establish a role for itself.

In their conclusion, Clarke, Fairbrother, and Borisov find that while the independent organizations produced some localized successes, no genuine national leadership of independent trade unions ever emerged. Most local branches of independent unions have been reduced to fighting rearguard actions against the onslaught of privatization that clearly favors management's rights over those of workers. Meanwhile, the official trade unions have retained their status and continue to be subservient to enterprise management.

The two books reviewed here at least implicitly place labor in a central role in recent events. Yet, there is no serious effort to weigh the relative importance of worker behavior against other sources of system instability and weakness. Both books make no secret of the authors' ideological sympathies with the workers' movement and the cause of workplace democratization. In the case of Clarke, Fairbrother, and Borisov, these sympathies apparently facilitated close personal contacts with independent union leaders, which proved to be the source of an impressive amount of detailed information about the inner workings of these movements.

Certainly, the weakness of the independent labor movement and its inability to mobilize Soviet workers in more than a few sectors demonstrated the success of the Soviet system in preventing this element of civil society from emerging, in marked contrast with the Polish case. Needless to say, no Soviet or Russian equivalent of the Solidarity trade union ever emerged to threaten the state.

**Embedded Autonomy: States and Industrial Transformation.** By Peter Evans. Princeton: Princeton University Press, 1995. 323p. $49.50 cloth, $17.95 paper.

Atul Kohli, *Princeton University*

This is a wonderful book, a real tour de force. It is essential reading for students of the political economy of development. It is also highly recommended to all those trying to understand the relationship of state and markets in the second half of the twentieth century, especially students of comparative politics.

The book seeks to analyze the role of states in both facilitating and hindering industrialization in contemporary developing countries. Moving away from the neo-utilitarian view that the best states are those that govern the least, Evans focuses instead on types of state intervention. To his mind, some states are better at facilitating industrialization than others. Why is this so? Evans develops his answer by undertaking a comparative analysis of three countries—Korea, Brazil, and India—that is focused on one sector of the economy, namely, computer industries.

Evans argues that states tend to be most successful as agents of economic transformation—as, for example, in Korea—when they exhibit the characteristic of "embedded autonomy," that is, a well-developed Weberian bureaucracy that gives the state a degree of corporate autonomy from social groups, on the one hand, and, on the other hand, an embeddedness in society, especially a good working relationship between bureaucrats and capitalists. Instead of hostile regulation or appropriation of private capital, states in such a setting prefer to facilitate the emergence and maturation of private firms; or stated in the new vocabulary Evans creates, successful economic states prefer the roles of "midwifery" and "husbandry" over those of "custodian" or "demiurge."

Working in the grand intellectual tradition of comparative political sociology that stretches from Max Weber to Barrington Moore, Peter Evans moves the study of states and economic development a giant step forward. This giant step consists of a persuasive demonstration that the neoliberal orthodoxy on states and economic development is simply not borne out by historical evidence and that when comparative evidence is examined closely, it turns out that states have played both positive and negative economic roles. Instead of ideological simplemindedness (some even mistake such simplemindedness for parsimonious theory), therefore, this more complex view provides a much better frame for discussing both successes and failures in post–World War II experience with deliberate development.

This is not to suggest that all that Peter Evans argues is persuasive or that he does not leave some important questions unanswered. For example, he focuses on the relationship of the state to private capital to explain successful state intervention. The explicit argument is that, instead of undertaking direct production, states do best when they nurture and cajole private entrepreneurs. Fine, but one does wonder how he would explain the relative success of Korea's steel industry, say, vis-à-vis that of India; both have after all been largely run as public enterprises. More important, questions of why states undertake different roles are not addressed directly. Why, for example, did the Korean state prefer facilitating the success of private entrepreneurs, whereas the state in India viewed its own capitalists with suspicion, preferring instead to control their activities? No single book can answer all the important questions. Nevertheless, attention to such questions would have forced Evans to take the political and historical context within which the states in these countries operate even more seriously.

Such questions aside, it is important to reiterate to all students of comparative political sociology and of political economy of development that a major new book has been published. Order it, read it, argue with it, and teach it in your classes.

**Nationalism, Ethnicity, and Identity: Cross-National and Comparative Perspectives.** Edited by Russell F. Farnen. New Brunswick, NJ: Transaction, 1994. 538p. $49.95.

William Safran, *University of Colorado at Boulder*

This collection of essays, written by members of a research committee of the International Political Science Association, deals with ethnicity and a number of related themes. It traverses much of the terrain of nationalism, identity, national stereotypes, political socialization, racism, authoritarianism, multiculturalism, and democratization. The book is a mine of information about these subjects and others touching upon them. There are particularly interesting discussions about the situation of minorities in postcommunist Eastern Europe, in which we are made aware of the impact of changing political contexts on national identity.

The editor's prefatory remarks and especially his introductory chapter on "Nationality and Public Policy" set the stage, and his concluding chapter summarizes the individual contributions, points out research problems, and sets out a sensible research agenda for the future. In fact, the introductory chapter, a *tour d'horizon* of the state of research on nationalism, ethnicity, and the elusive concept of identity, is so broad and comparative that it could stand by itself. However, the book as a whole has little internal coherence, except for the fact that it focuses heavily on the experiences of Eastern and Central Europe. Few of the individual chapters have a cross-national perspective, and there is no unifying theory. Instead, there is a considerable divergence in disciplinary orientation. This is not surprising, given the mix of the 21 contributors. Nine of them are from east-central Europe, five from the Netherlands, four from Germany and Austria, and only two (including the editor) from the United States; half of them are political scientists and the others are sociologists or anthropologists.

The introductory chapter refers to trends in the United States, Asia, the Middle East, Western Europe, and the formerly communist countries of Eastern Europe, and points out some of the major problems in each of them. In so doing, it touches upon such related factors as immigration, religion, poverty, and conflicts over educational curricula and some others that are often neglected, such as the media. The individual contributors do not necessarily analyze all or most of these factors, however.

Similarly, there are thumbnail definitions of many of these concepts, but they are of limited use because the concepts are used in different ways by the individual contributors. The chapters are uneven in length, depth, and quality. There are chapters based on solid empirical evidence, such as the one on "National Characteristics and National Stereotypes," one of the few cross-national studies, another on the development of democratic political identity in contemporary Ukraine, and a third that deals with the impact of class and ethnicity on identity among youths in Hungary. In contrast, some chapters are merely brief observations. Some essays, such as those on ethnic conflict in Bulgaria, the status of minorities in Poland, and xenophobia in East Germany, are too brief to be useful, while others are too tangential to the main subject of the book. Some chapters have misleading titles: the (generally excellent) chapter on Germany, which focuses heavily on "guest workers" and the acquisition of citizenship, deals with several European countries and with the European Union. Other chapters do not easily fit, such as the one on the role of the media in the formation of (ethnic and nonethnic) attitudes in the United States or the lengthy chapter on authoritarianism and the Cold War. Others again, such as chapter on the Germans' *Historikerstreit* and comparable attempts at coming to terms with the past, are not clearly related to the subject of ethnicity. Finally, still other chapters, such as those examining the role of the state in political and economic transformation or the role of intellectuals in the shaping of political culture, contribute little if anything to the building of theory about ethnonationalism. This is particularly the case when the intellectuals under discussion—e.g., Heine, Lem, and Bourdieu—have little in common in terms of chronology, context, and intellectual orientation.

There are discussions of educational theories, postmodernism, the nationalization of industries, class formation, *nomenklatura*, the authoritarian personality, genocide, and the political competence of the younger generation in a number of countries. There are repeated suggestions that these and other sociopolitical elements may have important bearings on collective consciousness. But the relationship between these phenomena and the development of national identity is brought out unevenly, if at all, and the individual essays, although for the most part informative, do not produce the promised "dialogue."

The book contains a significant number of references; unfortunately, their utility is limited because major theorists are omitted, e.g., Brass, Connor, Hobsbawm, Hroch, Kohn, M.G. Smith, or Furnivall, even where they are directly relevant to the subject under discussion. Thus, in a section on "imagined communities" (p. 178) no reference is made to Benedict Anderson.

There are some minor problems. For example, it is not made clear why, as the editor argues, " 'reasoned' patriotism and a 'healthy' national identity [should] be considered 'oxymorons' " (p. 446), "multiculturalism" is not always used in the same sense, sometimes connoting cultural-linguistic pluralism, and at other times, antiracism and affirmative action; and the impact of specific public policies regarding minorities on assimilation and/or heightened ethnic consciousness is not satisfactorily explored. Nor are the individual chapters equally attentive to the ethnic minorities in the countries they deal with, regardless of their present or historic importance. Thus, while the condition of the Sinti and Roma is given appropriate space in the chapter on Germany, the author of the chapter on determinants of Polish attitudes toward other nations probably considered the topic of Jews and antisemitism too sensitive to devote more than a few lines to it. There are numerous references to the relationship between political socialization and nationalism; but it is not made clear to what extent the socialization in democratic values (including civil liberties), especially in postcommunist countries that have recovered their national independence, has spilled over into acceptance of the rights of ethnic minorities and how the changeover to economic pluralism (i.e., a market economy) has contributed to an acceptance of ethnic pluralism.

**The Italian Revolution: The End of Politics Italian Style?** By Mark Gilbert. Boulder, CO: Westview, 1995. 204p. $44.95 cloth, $14.95 paper.

**The Crisis of the Italian State: From the Origins of the Cold War to the Fall of Berlusconi.** By Patrick McCarthy. New York: St. Martin's Press, 1995. 230p. $39.95.

Leonard Weinberg, *University of Nevada, Reno*

Evaluations by Anglo-American observers of Italy's democratic political system seem to have gone through three stages. The earliest judgments, dating from the 1950s and early 1960s, were essentially negative. Writers called attention to the astonishingly high levels of alienation and mistrust Italians displayed toward their post-Fascist political institutions. These institutions' performances hardly elicited much admiration. Observers noted the highly polarized party system, one in which Communists and Neo-Fascists achieved very substantial electoral support. The public administration was often depicted as archaic and unresponsive. Commentators also noticed that it took parliament more than a decade to establish such constitutionally mandated bodies as the constitutional court and regional governments.

There was a substantial reassessment during the second stage. In the aftermath of the *anni di piombo* (the years of lead) and the defeat of the major revolutionary terrorist groups at the end of the 1970s, Anglo-American writers reached more sanguine conclusions. They argued that, while the Italian system was seriously flawed, it functioned far better than its excessively cynical Italian critics would admit. Among other things, the observers called attention to the genuine power-sharing in the system that suggested that Italy had become a curious kind of consociational democracy. No party political groupings were completely without power. If the Communists did not hold cabinet posts in Rome, they controlled Emilia-Romagna and other regional governments in the center of the country. Furthermore, the party system had become depolarized as both left- and right-wing forces reconciled themselves to the democratic process.

Of course, while these more positive English language appraisals were being published during the 1980s, the Italian political system itself was reaching unprecedented levels of corruption, criminality, and dysfunction. It has been the public exposure of these characteristics during the 1990s and the concomitant political reaction that has caused Anglo-American writers to reach a third stage of evaluation, one that emphasizes the negative aspects of Italian political life but also identifies the forces of reform now at work in the system. Both Mark Gilbert's *The Italian Revolution* and Patrick McCarthy's *The Crisis of the Italian State* are third-stage books.

Their authors agree that the old Italian political order has collapsed and that the immediate causes of that collapse are known. Both Gilbert and McCarthy identify the collapse with two massive scandals, or sets of scandals, that were exposed by judicial investigations and accorded vast publicity from 1992 onward. First, the Clean Hands investigations, undertaken initially by Milanese magistrates headed by Antonio Di Pietro, revealed that many, if not most, Socialist and Christian Democratic politicians active in northern Italy, including parliamentarians and cabinet ministers at the national level in Rome, were the recipients of bribes and payoffs from virtually any business firm having dealings with the government. The money not only went to fill the coffers of the different party organizations but also to personally enrich the politicians who solicited the kickbacks. Such Socialist luminaries as Bettino Craxi, a former prime minister, and Gianni De Michelis, a former foreign minister, were accused of being shakedown specialists who had hidden billions of lira in foreign bank accounts.

The second set of scandals involved the role of organized crime in the political life of southern Italy. Investigators disclosed that in Sicily, Calabria, and Campania (the Naples area) intimate links had developed between criminal organizations—Mafia, 'Ndrangheta, and Cammora, respectively—and party organizations, usually but not always Christian Democratic. Typically, the criminal groups provided votes in exchange for public contracts awarded by party politicians to businesses controlled by the gangs. The parties were also able to provide organized crime with some protection from the threat of prosecution. Party-connected judges in Rome were encouraged to overturn convictions won by local-level prosecutors. Corruption was so extensive in some areas, Naples for one, that it became hard to tell where politics ended and organized crime began, or vice versa.

Gilbert and McCarthy agree, further, that the end of the Cold War contributed to the Italian crisis. In the absence of a Soviet threat and in the replacement of the Italian Communist Party by a non-Marxist Democratic Party of the Left, the need to maintain a Christian Democratic-led anticommunist alliance perpetually in power evaporated. Voters and members of the attentive public no longer felt constrained to ignore party political corruption by the ideological imperatives of earlier decades. According to Gilbert and McCarthy, what we have witnessed as a consequence is the collapse of *partitocrazia*, the whole system of clientelism that the major parties employed to penetrate and control vast areas of the economy and civil society in general.

Gilbert and McCarthy part company, however, in the strategies they employ in accounting for the Italian crisis. In *The Crisis of the Italian State*, McCarthy adopts a long-term perspective. He provides readers with an interpretive history of Italian politics since the end of World War II. In so doing, he seeks, in effect, to read history backwards in order to uncover those elements in the country's postwar experience that created the present situation. The tale he tells emphasizes the achievement of Catholic hegemony in the early years of the Cold War. In order to prevent a Communist victory in Italy, the Church put its substantial resources to work on the Christian Democrats' behalf. During the 1950s, however, Christian Democratic leaders sought to develop a party apparatus largely independent of the Church. To build such a party organization, these leaders provided highly tangible incentives. Through their control of government institutions, the Christian Democrats created an enormous web of clientelism involving jobs, public favors, and an array of public spending arrangements designed to maintain the party in power perpetually. Over time other parties in the anticommunist coalition came to share in the spoils. The public sector of the economy was even expanded specifically for the purpose of providing more benefits for the parties to award. Enter the Socialists. Under the exceptionally shrewd leadership of Bettino Craxi the Italian Socialist Party became an indispensable partner in the ruling coalition. Accordingly, Craxi acquired sufficient leverage to extract more jobs, bribes, and other favors for his party than its share of the vote would have warranted. In short, Craxi and his followers became greedy and so accelerated their own downfall along with the whole system of which they were a part.

While McCarthy stresses the long-term historical perspective, Gilbert's focus is much more contemporary. Although he expresses a few thoughts about the small-scale corruption of daily private life in Italy, he does not use them to build a cultural explanation for *The Italian Revolution*. What the reader does get is a series of snapshots. Each chapter provides the reader with an account of one aspect or another of the old order in crisis and of the new elements seeking to replace it. For instance, in the context of his discussion of organized crime's role in Neapolitan and Sicilian politics, Gilbert provides a wonderfully lucid and succinct description of the various techniques of vote stealing in ostensibly democratic elections. Anyone interested in these techniques would do well to read Gilbert's account of "the revolving ballot" and "the number that's not there." The author also uses his considerable powers of description in writing about the forces of reform, or at least change. Thus, just as he depicts the Christian Democratic and Socialist parties falling apart, with their former leaders (often Italy's erstwhile leading statesmen) going on trial or to prison or commiting suicide or fleeing into exile, Gilbert reports the appearance of Umberto Bossi's Northern League and Silvio Berlusconi's equally colorful *Forza Italia*. Unfortunately, these descriptions leave the reader with the impression that the new Italy these forces seek to bring into being likely will not be all that different from the old one.

Gilbert's description of *The Italian Revolution* suffers from a problem that accounts of the French and Russian revolutions do not share. In the latter cases, historians agree, more or less, that the Jacobins lost and the Bolsheviks won. But in Italy it is at present (1996) unclear what the outcome of the country's political crisis will be. As both McCarthy and Gilbert note, before the 1994 parliamentary elections there was a widespread belief that a reform-minded leftist coalition would win. Instead, the outcome produced a center-right government in which a party (the National Alliance) with a neo-fascist past held a number of important cabinet posts. Of course, none of this is the fault of Gilbert or McCarthy. They just happen to be writing about a country going through a highly volatile period in its postwar history.

Both *The Italian Revolution* and *The Crisis of the Italian State* may be criticized on two grounds. First, they are largely atheoretical, blow-by-blow descriptions of the old order's collapse. The accounts are certainly valuable in themselves, but neither author makes much effort to generalize or link the events to broader considerations. For instance, Robert Putnam's award-winning *Making Democracy Work* (1993) calls attention to the importance of "civic virtue" as a crucial condition for effective and democratic government. For reasons well beyond the scope of this review, Putnam observes that northern Italy abounds with this virtue, but that it is rarely found in the South. Despite this seemingly vital cultural difference, the abundance of civil virtue in the North did not seem to inhibit the region's party politicians and public officials from soliciting bribes and favors with the same degree of nonchalance as their counterparts in the *Mezzogiorno*. Neither Gilbert nor McCarthy have much to say about what would seem to be an important theoretical issue, one having to do with the relationship between civic culture and democratic politics.

Second, the two books are not comparative. This criticism may seem unfair. After all, both Gilbert and McCarthy set out to write about the crisis of *Italian* democracy. Why should they be criticized for accomplishing precisely what they intended? The answer is that other democracies along the northern rim of the Mediterranean—Spain and Greece in particular—have recently experienced similar crises involving bribe taking and other forms of corruption. Is the Italian experience or the experience of the Italian Socialist Party part of a more general pattern? It might have been helpful if the authors had been willing to place the Italian case in a comparative context.

Despite these criticisms, *The Italian Revolution* and *The Crisis of the Italian State* provide extremely valuable accounts of conditions prevailing in Italy's troubled democracy. English language readers with an interest in Italian politics might even find them indispensable.

**The Politics of Democratic Consolidation: Southern Europe in Comparative Perspective.** Edited by Richard Gunther, P. Nikiforos Diamandouros, and Hans-Jürgen Puhle. Baltimore: Johns Hopkins University Press, 1995. 493p. $60.00 cloth, $17.95 paper.

Enrique A. Baloyra, *University of Miami at Coral Gables*

This anthology about Greece, Italy, Portugal, and Spain is the first in a series that promises to examine how democracy emerged in these countries. It explores key features of their consolidated regimes, differences in economic structures and policies, changes in the nature of the state, and how socioeconomic development, cultural change, and democratic consolidation may have helped them finally overcome a notorious dualism between modernity and tradition. The central premises are that transition, consolidation, and persistence are different aspects of democratization (p. 3); that transition paths influence consolidation (p. 4); and that processes of consolidation deserve a more consistent theoretical argument established through rigorous comparative scholarship (p. 5).

In the Introduction, the editors beat some differences to death; embark in an argument about indicators; reduce the salience of trajectories of consolidation to the model of elite settlement and elite convergence advanced by Burton, Gunther, and Highley in *Elites and Democratic Consolidation* (1992); and fail to establish their case in more plausible terms than those already afforded by its basic intuitive appeal. A question lingers: What do these paths and trajectories consist of? In chapter 3, Linz, Stepan, and Gunther present a more convincing defense. But before getting there, the reader must ponder if Edward Malefakis fulfills the most difficult tasks assigned to anyone in the volume: demonstrate why Southern Europe is a region consisting of these four countries—a point not readily accepted beyond the safe confines of comparative politics—and establish this in terms of shared historical dynamics since 1814. Although he succeeds by the narrowest of margins, his may be the chapter with greatest appeal to other comparativists.

Large claims are made in chapter 3, inspired by findings of Linz and Stepan that are discussed in *Problems of Democratic Transition and Consolidation* (forthcoming) and sandwiched between very astute comparisons with South America and Eastern Europe. The first of these is that the nature of the previous regime may impose simultaneities on the reformers, who must build institutions on too many fronts, and that this legacy has the greatest impact on consolidation. Second, concerning threats to stateness posed by separatism, it is asserted that national political identities are less primordial and more contingent than anticipated. A convincing case is made using the Spanish "transacted transition," discussed with much less reverence than is usually the case in the literature. Through it all, there runs a practical demonstration of why Greece, Portugal, and Spain are consolidated polyarchies in terms of structural, attitudinal, and behavioral criteria. But the authors' terminology can be distracting; for example, an elusive distinction between "hierarchical" (senior) and "nonhierarchical" (field-grade) military obscures a fairly obvious point; low quality democracy having fewer "degrees of freedom" is a lyric transgression.

The conclusions of other contributors are predictable: the international context was mainly helpful (Geoffrey Pridham), the regimes were legitimate and consolidated in the mid-1980s (Leonardo Morlino and José Montero), executive stability helps consolidation (Gianfranco Pasquino), and interest intermediation has moved closer to neocorporatism (Philippe Schmitter). Three others must be set aside, not only for their substantive importance but also because they deal more explicitly with discontinuity. In a volume that goes to great lengths to accentuate the importance of paths, stages, and sequences to provide "explanations what" (as in Dray's 1959 essay "Explanations What in History"), the essays on the military, social movements, and political parties emphasize process and conditionality, which cannot be obviated in searching for the "why" of transition and consolidation. These three essays evade some conceptual traps relating to timing and suggest that we may think of stages in terms of cycles or iterations. This notion is important because stages only become apparent through discontinuity, and discontinuity is possible only through certain combinations of agency, process (more than a cumulation of discrete strategies), and conditionality. Not everything is possible at any given time.

Felipe Agüero dwells on the conditionality posed by outgoing regimes that were militarized. Initially, he follows convention by linking this conditionality to different trajectories and arguing the merits of his own typology. More important, he returns to initial conditions to show how these impose constraints on the strategic perspectives of actors trying to deepen consolidation in the matter of civilian supremacy. Despite constraints, more of the same can be different.

Sidney Tarrow accepts that the proper emphasis is choice, not determination, but argues that *someone* must transform structural factors into action through political choice. He writes that every democratic "episode" can be seen as a "cycle" of mobilization and "strategic interaction," and shows that the strategic choices of mass publics induce elites to move either toward democracy or in some other direction. His counterfactual is how elite consensus could have possibly saved the day in Italy (1919–22), given that parliamentary cleavages prevented moderate alliances, and when social turbulence contributed to "leftward contagion" and extraparliamentary fascism. By contrast, the Spanish transition (1975–78) benefited both from the absence of conditions that once helped drive the Republic to civil war—separatism, labor strife, and class war in the countryside that turned conservatives into reactionaries—*and* from a prolonged struggle. These did not predetermine Spain's triumphant success, they simply posed more favorable conditions for interactions between masses and elites, and for elite transactions. It was no cakewalk. Even "better" could have turned out much worse.

In his chapter (justifiably the longest), Leonardo Morlino examines the complex relationship of electoral behavior, party system stability, legitimacy, and consolidation. Consolidation enables him to engage in "normal sciencing" and to rely on standard techniques and communicate with mainstream disciplinary concerns, handled here admirably. But Morlino's main virtue is showing how a comparable outcome—electoral stabilization—resulted from conjunctions of potentially contradictory effects: Electoral success and party organization are not closely related, party polarization and moderation can cut both ways, and party stabilization may be neither sufficient nor necessary for democratic consolidation. But if legitimation has not gone far enough to consolidate the regime, party stabilization may be indispensable. Nothing is left to deductivism. His account of the radical transformation of the contemporary Italian party system shows that an analytical strategy rooted exclusively in stages cannot possibly predict discontinuity and is only sensible ex post facto.

The generic study of transition and consolidation can rely on the narrative tradition imposed by Linz, Stepan, and their closest collaborators for the indispensable raw material. Efforts to move beyond may evolve taxonomies based on stages or on the rationality of different games. These approaches are not mutually exclusive, and both may benefit considerably from greater clarity about the interaction of agency, process, and conditionality.

**Policymaking for Conservation in Latin America: National Parks, Reserves, and the Environment.** By Jack W. Hopkins. Westport, CT: Praeger, 1995. 216p. $55.00.

Eduardo Silva, *University of Missouri-St. Louis*

The general literature on environment and development focuses heavily on the diagnosis of specific problems in order to advance prescriptions for their solution. Yet, few studies examine the crucial question of the political factors that affect the translation of those prescriptions into public policy, beginning with their placement on the policy agenda, continuing to policy formulation, and on to implementation. This is even truer for the area of conservation issues, which has become increasingly salient as the concept of biodiversity gains ever-wider currency in international and domestic fora in the wake of the 1992 United Nations-sponsored Earth Summit in Brazil. Much of the literature on parks and reserves in developing countries focuses on descriptions of geographical areas or problems in park management and financing. Increasingly, attention also is lavished on novel ideas for viable parks and reserves and how to finance them. Widely discussed policy options include the identification of key biomes that need protecting to efficiently preserve biodiversity, the institutional strengthening of park services, streamlining and harmonizing legislation and agencies, biogenetic prospecting by transnational corporations, debt-for-nature swaps, and the need to provide economic opportunities for impoverished rural communities—the livelihood question. As in the case of the broader literature on environment and development, notions about whether these worthy prescriptions will ever be supported by public policy are remanded to the nebulous, mysterious, and sometimes unsavory underworld of "political will."

Jack W. Hopkins's book is a welcome attempt to address this gap in the general literature on conservation and environment in developing countries through the application of a policy-stages approach to policymaking in three Latin American countries: Argentina, Chile, and Costa Rica. Hopkins argues that conservation policy in these countries is the result of complex conjunctures of international, domestic, structural, and ideological factors—factors that he deems particularly important for understanding the agenda-setting and policy-formulation stages of the policymaking process. The specific mix of these factors in each case ultimately can guide us in assessing whether policy choices are moving countries toward the creation of "living" protected area systems or "dead" ones. The former are parks and reserves embedded in the web of socioeconomic relations of the surrounding region. Here the problem is approached from the point of view of people *in* nature: When parks become part of the solution to the livelihood concerns of local communities they stand a better chance of survival from settler encroachment and land-use change. By extension, policies based on the idea of nature without people, preservation for the sake of preservation, are not likely to succeed, given the press of the surrounding impoverished population.

In a novel departure for Latin Americanists, Hopkins also argues that public awareness of conservation issues is key to getting protection on the policy agenda, to pushing through legislation, and as a defense against encroachment. Of the three cases, Costa Rica exhibits the highest level of conservation policy, and it has the highest level of public awareness; Argentina, by contrast, scores lowest on both measures; Chile lies somewhere in between. In another departure from conventional Latin American studies and contributing to emerging work on the importance of the rule of law, Hopkins uses the Chilean case to show that the judicial sphere for conservation is a crucial complement to effective citizen action.

The author then points out that ironically even relatively low levels of public awareness suffice to get results, since nature protection is not high on the list of policy priorities for any Latin American government—economic growth, employment, and political stability rank much higher. Under these conditions, a few determined people inside and outside of government can shape legislation shielded from the press of private and public interests that affect more significant issues. This helps explain why the three cases have relatively large areas officially under protection. But, alas, the low salience of the issue also means that government agencies charged with managing the parks and reserves receive little funding beyond minimal salary requirements, thus adversely affecting policy effectiveness.

Here is where international agencies can play an important role, as the Costa Rican case illustrates. Their funding capability and their more advanced ideas about conservation can decisively shape the direction of policy away from the strictly preservationist focus that imbues most of the park personnel in Latin America. They also provide crucial support in policy debates and complement implementation efforts. Of course, domestic structure—especially long periods of closed, repressive military regimes (Argentina and Chile) versus more open and established democratic systems (Costa Rica)—have also affected the relative position of the cases.

Hopkins's general approach offers solid points of departure for future work. First, by inserting the issue in the larger social, economic, and political contexts that affect it, he shifts attention away from the usually narrow focus on park management problems and from the tendency to blame the lack of action on weak political "will." Second, the book is suggestive of the potential advantages of placing analysis at the intersection of international system and domestic structure approaches rather than limiting it to one or the other, which is currently the most common approach. This is particularly important for an issue area in which international actors play a strong role in the context of low issue salience. Third, from a view sympathetic to protection, Hopkins takes on the debate between preservation and livelihood for local communities and its significance for the implementation of conservation policy. This debate lies at the core of divisions among actors in this issue area, yet it is often avoided in scholarly publications.

The book's strength is that it presents a wealth of information about and interesting insights into the policymaking process for conservation in Latin America in general and the three cases in particular. Its main shortcoming is the lack of a strategy for qualitative comparative analysis. There is no clear specification of the problem Hopkins wishes to analyze and how the cases vary along that dimension. Sometimes the various stages of the policymaking process are the focus of analysis, while at other times it is the general body of legislation for protection, its orientation, and the efficacy of its implementation. Environmental policy is also discussed, and at the end he introduces the concept of policy styles. In and of themselves these discussions are all interesting. But the book would have been stronger if they had been presented more precisely as dimensions of a large and complex problem.

The explanatory factors also require more careful specification, especially the international ones. A greater effort to systematically tease out the relationship between the large set of multifaceted variables and outcomes would have enriched the book and given more coherence to its central arguments. One feels an opportunity has been missed to methodically explore hypotheses about what happens to conservation policy in the presence or absence, or the relative strength or weakness, of the proposed explanatory factors. The use of evidence as illustration rather than as part of a strategy for comparative analysis contributes to the problem. These observations are not a call for universal theorizing. One can respect the historically particular, as Hopkins refreshingly argues for, and still do systematic comparative work. Because they deal with change, methodologies drawn from historical sociology, explaining alternative development paths, or the work on free-market economic restructuring offer interesting points of departure for qualitative comparative analysis in environment and development.

**Professionals against Populism: The Peres Government and Democracy.** By Michael Keren. Albany: State University of New York Press, 1995. 147p. $16.95.

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Michael Keren has written a first-rate, bold, and (in the best sense) provocative work of scholarship and argumentation on the appropriate role of the knowledge professions in a democracy, with Israel providing his examples and case studies. The controversy that his arguments are bound to spark should be a productive one.

The central premise underlying the book is that as a result of enormous strains produced by the unending Arab-Israeli conflict, Israeli democracy is a fragile one beset by powerful forces of populism. In the Israeli context, Keren asserts, populism is jingoistic, fundamentalist, emotional, authoritarian, and prone to violence. The role of the learned profession, or as Keren calls them, "the knowledge elite"—academicians, scientists, journalists, psychologists, the intelligentsia—is to maintain its professional independence while at the same time actively participating in the work of government by shoring up pluralist democracy against the populist forces of irrationality and darkness. The professionals must not only put their particular knowledge and expertise to work on behalf of society but also represent and defend the values of rationality, moderation, and pragmatism.

At a more theoretical level, Keren proposes three models for the relationship between the knowledge professions and the state. In the autonomy model, the professions are primarily interested in maintaining their independence and distance from the state: "Knowledge maintains a distance from power," as the author sums it up (p. 104). In the cooption model, professionals eagerly participate in the affairs of state, gaining a variety of individual status, financial, and psychological benefits, but in so doing they sacrifice their independence and critical function and fail to productively challenge existing policies and premises: "Knowledge is coopted by power." The third and preferred model is the "exchange" model, in which there is a healthy "balance between knowledge and power." In this model, the professionals do not simply adopt and serve the political goals and agenda of the state. Rather, they use their own expertise and knowledge to develop an independent political program and then enter the political arena to work to achieve their goals while at the same time maintaining a sufficient distance from the politicians to perform their historical role as critics of power and society.

The empirical core of *Professionals against Populism* consists of three case studies representing these three models of professionalism. They are all drawn from the 1984–86 period in Israel when Shimon Peres, the leader of the Labor Party, served as prime minister. Keren evidently had close ties with Peres and the people around him and was given access to Peres's private papers, which he has put to excellent use in these rich studies.

The autonomy model is represented by the relationship of the legal profession to the Peres administration during a government coverup of the summary execution of two captured Palestinian terrorists by high-level police officials. Though successful in helping to keep the issue out of mass politics ("off the street"), the legal professionals, focusing on maintaining their autonomy from the government, failed to uphold their responsibility to insist on the strict rule of law.

The exchange model is represented by the relationship of Israeli economists and the government in the successful 1984 rescue of a collapsing Israeli economy. Working in close conjunction with the Peres administration and government bureaucrats, the economists ignored popular pressures and maintained their professional independence, yet with great effectiveness joined forces with political leaders to curb inflation and stabilize the economy.

As a specialist in international politics, I cannot evaluate the accuracy or persuasiveness of Keren's analysis in these two cases. However, I am more familiar with the author's third case, representing the cooption model and the failure of professionalism—the relationship between academic security experts and the Peres government in 1984–86, during which time Peres sought to reach an Israeli-Jordanian agreement over the Israeli-occupied West Bank and the Gaza strip.

Under the terms of the power-sharing agreement reached by the Labor and Likud parties in 1984 (necessitated by the failure of either to win a working majority in the 1984 elections), Peres served as prime minister in 1984–86 and Yitzhak Shamir of Likud did so in 1986–88. As Shamir had no interest in a compromise peace, Peres sought to create a fait accompli during his term. However, at that time even Peres was unwilling to deal with Arafat and the PLO or to reach a territorial compromise with the Palestinians, so he sought to deal only with King Hussein of Jordan.

In pursuit of this goal, the Labor Party sought the active participation of Israeli academic strategic experts, and during this period there were very close ties between the academicians, the military, and the government. However, as Keren tells the story, the academicians did not so much influence the politicians and military as they were coopted by them: Eager to share in at least the trappings of power, the knowledge elite failed to challenge the narrow premises of the power elite. Peace could have been reached, Keren argues, only if the Israeli government had been willing to recognize the centrality of the Palestinians, negotiate with Arafat and the PLO, and move toward Israeli withdrawal from the occupied territories and the subsequent establishment of a Palestinian state.

In the end, Peres's initiative came to nothing; Keren places heavy weight for this failure on the coopted academicians whose only effect was to legitimize a sterile and dangerous status quo. While I think that Keren is right in his assessment of Israeli policy, I have a number of reservations about his "cooption" argument. First, it is an oversimplification to argue that it was their eagerness to share power that accounts for the failure of the security professionals to press for what they presumably knew was required to reach peace. Perhaps the argument is at least partially correct for some, but one can be confident that most of the "experts" brought by Peres into the peace process genuinely shared—however erroneously—his preferences for bypassing the Palestinians and making as few concessions as possible. Those few academicians who went beyond Peres's narrow and shortsighted position were at that time marginalized and excluded from the peace process.

Moreover, during this period the constraints posed by Israeli domestic politics and public opinion almost certainly would have blocked any agreement based on dealing with the PLO and an Israeli withdrawal from the occupied territories. Shamir had the power to veto any Peres initiative, and he would not have agreed even to a deal with King Hussein, let alone to a more far-reaching settlement—and Israeli public opinion would have supported him, not Peres. While acknowledging these constraints, Keren implicitly downplays their importance in working out the implications of his professionalism model.

Finally, Keren might at least have mentioned the rise of "the new Israeli history" movement since the 1980s: the emergence of a group of Israeli political scientists, historians, and sociologists whose scholarship has effectively challenged much of Israel's historical mythology, especially concerning the Arab-Israeli conflict. To the extent that the prospects for the peace process today are more hopeful than in the past, at least some credit should be given to this new "knowledge elite."

Undoubtedly, many readers will take strong issue with other parts of Keren's argument: his obvious political preferences; his assessment of the Arab-Israeli peace process; his insistence that true professionalism requires combining "professional ingenuity with political consciousness," indeed, with activism in the work of government; and his "elitism" and rejection of mass or populist democracy. However, I happen to share all of these views, including Keren's all but explicit identification of the Likud party with the forces of darkness. Indeed, because the Likud's commitment to the Arab-Israeli peace process is not clear, in a sense Keren's book is even more timely than he could have expected at the time it was written—though at the same time I imagine he would be considerably happier with the present role of the Israeli security professionals, particularly including the new historians.

Notwithstanding my own reservations, Keren has largely succeeded in the rare feat of combining theory with history, description and analysis with normative prescription, and scholarship with a passionate commitment to political change.

**Social Justice and Political Change: Public Opinion in Capitalist and Post-Capitalist States.** Edited by James R. Kluegel, David S. Mason, and Bernard Wegener. New York: Aldine De Gruyter, 1995. $55.95 cloth, $27.95 paper.

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The editors and contributors to *Social Justice and Political Change* have made a significant addition to our knowledge of public opinion in both the Western capitalist democracies and among formerly communist states in Eastern Europe and the Soviet Union. Readers will find a virtual treasure trove in this enormously ambitious work that explores questions of social and economic equality over a range of political, social, and economic contexts.

An outgrowth of the International Social Justice Project, this volume is the culmination of painstaking research that in the late 1980s and early 1990s produced a thorough cross-national survey covering both a number of former communist nations, including Bulgaria, Czechoslovakia, Hungary, Poland, and Russia, and also both Germanys, Great Britain, Japan, the Netherlands, and the United States. Thus, it provides a nearly unique opportunity for cross-national contrasts and comparisons, and expands upon the work of Finifter and Mickiewicz ("Redefining the Political System of the USSR: Mass Support for Political Change," *American Political Science Review*, 1992, pp. 857–74), and others.

The authors avoid the possible temptation to simply provide a country-by-country analysis. Rather, in the spirit of Przeworski and Teune's classic *Logic of Comparative Social Inquiry* (1970), the volume is organized along theoretical and conceptual lines that address two key questions in the research literature: "What do justice beliefs explain?" and "How do we explain justice beliefs?" (p. 6). Part I seeks to answer the former question, while parts II, III, and IV are concerned with the latter.

Part I begins with a chapter by Adam Swift and colleagues, "Distributive Justice: Does It Matter What People Think?" Presenting data from Britain, the United States, and (then) West Germany, they first explore questions of *what* citizens think about justice and equity issues and, second, the linkage between those attitudes and actual behavior. They suggest that there is a coherence of beliefs and attitudes across all three countries that can determine behavior. Although attitudinal structures obviously are not totally determining, there are important linkages that deserve further exploration. Kluegel and Miyano (chap. 4) expand upon these questions, concluding that support for welfare state policies is generated by a complex matrix of sociodemographic and political structural features of the political system.

David Mason's contribution, "Justice, Socialism and Participation in Post-Communist States" (chap. 3), addresses issues of ideological and attitudinal structures in the former Eastern Bloc. While citizens appear to support capitalism and its attendant material benefits, the author emphasizes that a coherent civic philosophy has yet to emerge. This political vacuum, it is safe to say, continues to have enormous practical policy implications, as Russia has moved with great hesitation toward stable market institutions. Another disturbing feature of Mason's findings are the high degree of "frustration, futility and alienation" (p. 67) expressed by citizens, which may help explain Paul Hockenos's recent work *Free to Hate* (1993), where he examines one use of extremist movements in Eastern Europe following the communist collapse.

Successive chapters seek to explain how justice beliefs develop. Chapters are organized on the basis of major contending theoretical perspectives. Contributors address the respective roles of microjustice beliefs (involving perceptions of how an individual is treated) and macrojustice beliefs (which concern how citizens as a whole are treated) (p. 8).

Although space considerations do not permit a detailed discussion of each chapter, this reviewer was especially struck by Alwin et al. (chap. 5) who find important differences between capitalist and postcommunist politics in the relationship between actual personal income and perceptions of deserved income, with the linkage being much stronger in the West. Given some of the earlier findings by Mason, these opinion profiles, if they hold, may not bode well for the future democratic evolution of postcommunist states.

The role of macrojustice beliefs is explored in parts III and IV, with the former explicating the role of ideology. In a treatment of one of the most familiar dimensions of this discussion (chap. 8), Kluegel and colleagues examine the respective roles of social and systemic versus individual explanations for individual failure, a debate that resonates in the United States and elsewhere as sometimes sweeping changes in social welfare programs are debated. This theme also carries over to chapters 9 and 10 in this section, particularly the latter by Wegener and Liebig, where the role of dominant ideologies in shaping norms of distribution future are developed.

The final section addresses the issue of the role of social structure. Wegener and Liebig explore differing conceptions of social justice in East and West Germany. An illuminating and fascinating contribution by Davidson et al., "The Caring but Unjust Woman . . . ," suggests an important role for rational and narrative beliefs in shaping perceptions of social justice.

As in any hugely ambitious project of this type, one can always identify potential minor flaws. Given the cultural gulf between some systems, one wonders whether many of the concepts that researchers attempt to capture in the use of identical questionnaires adequately does the job. Of course, only further research will tell the tale. And it may be useful to emphasize that, particularly with regard to the former communist states, the survey was taken at a time of almost total upheaval and disruption, academic, political, and social. Whether the findings gleaned from survey instruments will hold up over time is a matter not just for scholars, but for policymakers as well.

In spite of the above caveats, *Social Justice and Political Change* does a superb job of integrating the political and philosophical works on justice with the empirical research tradition and theories of comparative politics. Thus, this work represents what one set of contributors, according to Elster, acknowledge as the requirement for theories of justice to be supported by empirical foundations (p. 18). As such, it deserves to be in the syllabi of both undergraduate and graduate courses in public opinion, political theory, and comparative politics, and is almost certain to be well cited by researchers working in these areas. Future research experts, we hope, will allow us to build upon this timely work.

**Cash, Crisis, and Corporate Governance: The Role of National Financial Systems in Industrial Restructuring.** By Victoria Marklew. Ann Arbor: University of Michigan Press, 1995. 260p. $44.50.

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This book explores how national finance affects firm survival and adaptation in times of crisis. The idea is not new; it was first systematically explored by Zysman more than a decade ago. What Marklew adds to this literature is attention to the bottom-up approach of examining the structure of national financial systems by looking at them from the firm's point of view. More specifically, three main factors are found to be critical to a firm's adjustment—the cost and availability of capital and the attitude of the providers of capital. The author examines four companies with private and public ownership structure: British Steel, British Leyland, Usinor-Sacilor, and Peugeot. Her analysis enables her to reach two basic conclusions. First, Thatcher's Britain and Mitterrand's France had more in common than is widely acknowledged. In both cases, the state responded to industrial crises quite similarly despite significant differences in ideology and institutional constraints. Second, the differences between public and private sectors are becoming increasingly blurred. What matters in government-industry relations are not labels but the financial structures of the firms themselves and the national systems that affect them.

The author illuminates the increasingly sophisticated ways that governments help both public and private firms. The capitalist system is indeed changing from one where ownership was key to one where ownership does not matter. In this way, Marklew joins a host of other analysts who have shown that labels such as "public" and "private" are unable to capture the essence of this transformation. One goal for the next generation of comparative political economists will be to uncover the characteristics of the new environment that make ownership irrelevant. Moreover, it is imperative to look into the technologies of the public sector to illuminate the instruments of control that governments use and the ways that these same instruments become tools of survival and adaptation in the hands of corporate managers. In this environment, the author rightly concludes, the government's ideology plays no role.

Fair enough, but is this the end of ideological attachment to ownership? Not really! The irony is that ownership is made irrelevant only by ideological commitment to a particular form of ownership. Changes in ownership in Britain and France have been made possible only by arguing that ownership is extremely important and that only private ownership will make firms more competitive. At the same time, governments find themselves having to aid firms regardless of their public or private status. So does ownership and, by extension, the government's commitment to it matter, or is the financial environment within which firms find themselves more important to their performance? This is not an easy question to answer, but the simple answer is that ownership does not matter so long as all firms are private. The new capitalist system makes ownership irrelevant only by making all firms private. This is an answer that has very strong ideological dimensions. A close look at the aims of Mitterrand's and Thatcher's governments reveals that industrial policy differed greatly based on the willingness of each government to experiment with mixes of public-private financing. The reasons for such differences are based on legal instruments and state willingness to use them or create new ones. There are statutory limitations on multinationalization and product diversification that make British state-owned companies far more financially dependent on the public purse than French companies. Consequently, the options available to corporate managers are significantly different in the two countries and between public and private companies in the same country. Moreover, Thatcher was far more unwilling to continue managing state-owned enterprises (SOEs) after they were rescued than was Mitterrand. She put a squeeze on corporate finances making it close to impossible for British SOEs to compete if they remained in the public sector. Perhaps the key to exploring the impact of ideology and a government's commitment to state ownership lies not so much in questions of whether governments are willing to prop up ailing companies but rather in how and what influence they intend to retain after company finances are brought back into shape. In the new system, one could conclude—in contrast to the author—that the firm's financial environment matters so long as ownership does not fluctuate.

This book is well argued and well researched. The study could benefit, however, from more explicit treatment of two areas. The first is methodological and afflicts most comparative quasi-experimental designs. Has the dependent variable (firm adjustment) been sufficiently delimited to include all or at least most variations? In other words, are there any other adjustment strategies not explored by the study that would reveal information that qualifies or even refutes the conclusions? How does the author know she has not committed sampling error? This is, of course, a problem of all bottom-up approaches that seek to make generalizations from firm-level analyses. It is very difficult to know whether one has marshalled sufficient evidence to arrive at solid conclusions. True, the author acknowledges this possibility (p. 171), but a more explicit treatment of the issue is needed much earlier in the study.

Second, the conclusions could have touched on an additional area of theoretical importance: the nexus between political economy and public policy. There is currently little dialogue between the two fields in comparative studies although each has considerable insights to offer to the other. For example, since the findings suggest that mixed economies are evolving into more complex public-private partnerships, can this insight be tied to theories of policy networks? If so, how? Can we go beyond state and market issues into theories of how policies are actually made from a bottom-up perspective? Most theories of policymaking tend to be compartmentalized into narrow domains, such as education, health, or transportation policy. Consequently, their insights apply solely to education, transportation networks, and the like. This is an opportunity to transcend this narrow focus and draw conclusions that apply to several industries at once. If financial structures of various firms and the networks in which they are embedded are critical variables in understanding adjustment strategies, it follows that a theoretical treatment of these networks can serve as a bridge that unifies theory in diverse sectors. What do these financial networks look like? Are they fragmented and becoming even more so? Do networks in some countries contain more financing options than networks in other countries? The answers are contained in this study, but they are unfortunately lost in the details of the case studies. They could have been brought together in the concluding chapter to be used by policy analysts as a springboard to delve further into the nuances of comparative policymaking.

This is a very good book. It makes a strong case for linking the micro to the macro in an effort to reveal how adjustment works from the bottom up. The book also serves another useful purpose. It bridges political science approaches that are almost exclusively preoccupied with state behavior and business school approaches that examine corporate behavior. Each perspective illuminates different aspects of the same phenomenon, but the disciplinary distinction between politics and business remains sharp. We need more empirical and theoretical analyses to find out how the two may be combined to yield a more complete understanding of political reality.

**Fundamentalism Comprehended.** Edited by Martin E. Marty and R. Scott Appleby. Chicago: University of Chicago Press, 1995. 522p. Vol. 5 of *The Fundamentalism Project*. $45.00.

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The American Academy of Arts and Sciences commissioned the Fundamentalism Project in 1988, and this is the fifth and final volume. To date, the *APSR* has not reviewed a single book in the series; and this is almost inexplicable because a giant in our discipline, Gabriel Almond, was prominent in its design and implementation, because religion and politics is an APSA subfield, because fundamentalism is an extremely significant political force, and because social scientists dominate the series, contributing 15 of the 19 essays in this volume.

The project is monumental, with respect to both productivity and organization. Around a hundred scholars in many disciplines contributed articles; nearly all were authoritative and most were fascinating, too. Like Jacob they labored seven years for their prize, and during that time six general conferences, six international preliminary assemblies, and perhaps a dozen teams met. Graduate seminars at Chicago usefully dissected early drafts. Study field trips were subsidized. There were also spinoff projects: a fine volume, *Fundamentalisms and the Gulf Crisis*, edited by James Piscatori (1992), a newsletter, and a television and radio series entitled *The Glory and the Power.*

The editors displayed imagination, intelligence, and enormous energy in mobilizing their considerable resources. The first volume, *Fundamentalisms Observed*, employed fourteen case studies of movements within Christianity, Judaism, Islam, Hinduism, Buddhism, Sikhism, and Confucianism to demonstrate important common features, but as the term *fundamentalisms* reminds us, a "family" is being discussed. Unlike some families, such as Marxism, members came from distinctly different traditions and are fixed forever in one part of the family. *Fundamentalisms and Society* (vol. 2) examines relationships to family, gender, education, science, and technology. *Fundamentalisms and the State* (vol. 3) evaluates economic dimensions and quests for political power both by legal and violent means. *Accounting for Fundamentalisms Movements* (Vol. 4) discusses how internal characteristics are altered when religious radicalism moves into or away from the outside world. Associate editors wrote integrating analytic chapters and organized teams to weave related case studies together.

For reasons not discussed, *Fundamentalisms Comprehended* is organized differently, with less satisfying results. The teams and intermediary integrative analytic chapters are scrapped. Individuals are on their own to make whatever comparisons they see fit. Articles in Part 2 compare themes across traditions, and in Part 3 movements within the same tradition are considered. But the logic of the choices made is neither explained nor obvious. Samuel Heilman, for example, provides a penetrating analysis of scholars in Jewish and Islamic fundamentalist groups. But why are only these traditions considered? Valerie Hoffmann provides a valuable psychosocial profile of Muslims in five states, but is this a subject relevant only to Islam? Part 4, euphemistically entitled "New Approaches and Interpretations," may be more accurately described as "essays one had to include because they were good in themselves and/or written by someone too important to ignore." Thus, Gideon Aran examines humor among Jewish fundamentalists, Wayne Booth explains the rhetoric of conversion narratives, and the late Ernest Gellner makes a penetrating attempt to show why Islam has more staying power than Marxism to demonstrate his thesis that secular religions are generally fragile. With respect to these three sections, the volume loses its unity or the real structural distinctiveness of the project to become just another collection of very good essays on an important subject. This may be why these sections are not integrated in what the editors call the project's "capstone statement," Emmanuel Sivan's impressive phenomenological study "The Enclave Culture" and four final chapters jointly written by Sivan, Almond, and Appleby.

The "enclave" is a fragile opposition community "predicated upon voluntary membership and equality," and held together by moral suasion. It requires a demanding lifestyle emphasizing the virtuous character of members and deep anxieties concerning the temptations outside. Sivan discusses religious traditions, showing how each shapes its own enclaves. The special feature of the fundamentalist enclave is its response to secularism, which it understands as encouraging "instant gratification" and a belief that only human desire legitimizes value. Members understand themselves to be living at the end of an era, when virtuous sufferers—like those who live at the climax in the Marxist view of history—will be rewarded. Supernatural interventions will occur in history's climax just as they did in its beginning. Enclaves want to expand, but the cost could be the quality of the group, and since even survival, at least in the short run, is problematic, commitments to boundary maintenance become paramount and decisively shape willingness to take risks. The enclave's voluntary nature, the premium on equality, and belief that extraordinary demands must be made upon members provoke schisms and internal controversies, "hamper decision making and render authority ambiguous."

The final four chapters offer an analytical scheme, based on this essay and those in the four preceding volumes. Five ideological and four organizational properties, appearing in various configurations, are distinguished. Ideology stems both from deep anxiety that religion is on the verge of extinction and efforts to blend reinterpreted aspects of tradition with elements of secularism. Beliefs in the inerrancy of sacred text, moral manicheism, and messianism abound. Organizational properties are supposedly those discussed by the enclave essay, but the emphasis on authoritarian structure is not reconciled with an earlier stress on persuasion and equality.

In the last chapter, a model is designed to explain the origins, growth, and decline of movements; and then the original cases are reexamined to show how they relate to the model. The interaction of movements with their environments are analyzed with a "grid of three interlocking levels of analysis—long-term economic, social, religious, and political structures" that are the general conditions, "triggers," or events that inspire movements, and the internal dynamics of those movements. A fundamentalist movement has several strategies available (world conqueror, trans-former, creator, and renouncer), and the model is used to help explain those choices.

To the outside world, fundamentalists are known largely through political activities, and it seems fitting that the authors, who never lose sight of the importance of religion, return to politics in the final chapter. The political context is distinguished in four ways. The first two concern whether the movement operates in nondemocratic (authoritarian) or democratic states. In authoritarian regimes, fundamentalists can scarcely operate; nonetheless, they cannot be eliminated. When authoritarian regimes relax, fundamentalist movements expand so rapidly that frightened governments mobilize their coercive powers again. No equilibrium emerges. Equilibriums are unlikely in democratic states, too, for different reasons. Here fundamentalists seek alliances with nonfundamentalist elements, thus predictably diluting their identity and compelling a retreat to original enclaves to wait for another historic opportunity to expand. The existence or nonexistence of religious communities sufficiently distinctive to claim a separate ethnic identity are the two other major political conditions. If they do exist, fundamentalism sharpens existing antagonisms between different religious communities, dramatically enhancing possibilities for violence and leading to state partition and/or destruction.

The Fundamentalism Project is an important achievement; no one will discuss fundamentalism without seriously considering its products. Individual essays are and will continue to be landmarks. The effort compares very favorably with an earlier collaborative effort to discuss secularism, Almond and Coleman's *Politics of the Developing Areas* (1960). The earlier case studies soon lost their value because contributors were so consumed with efforts to impose a teleological scheme that they supplied no information. This time the scheme comes after the empirical materials are assembled, and it is a much more cautious one to boot.

The Fundamentalism Project did run into problems and was abandoned earlier than anticipated. Although not mentioned here, a conference for a sixth volume was held; the promising papers were not published, and since they were evaluations of state policies, the failure was particularly significant to political scientists.

Alas, both the money and the energy ran out, a depletion that had other consequences. Without the intermediary associate editors, the burden on those responsible for the synthesis was probably too great. One person could not deal with the enclave theme in all religions, and consequently the capstone enclave theme is deficient with respect to Asian religions. There was sometimes a failure to recognize, let alone clarify, differences between the synthesis and individual arguments. Essays in volume 2, for example, emphasize messianic components in Sikhism and Buddhism, but we get a very different picture in the capstone statement. Copy editing failures and incomplete references (see pp. 193 and 194) even suggest that not all contributors read the analytic essays. One, for example, has the editors claiming that messianism was not a prominent feature of fundamentalism, citing an essay in the book that in fact does not appear there (p. 181)!

The synthesis fails to discuss issues that seem critical. Since fundamentalism is a reaction to secularism, one would have thought that the degree to which a community was secularized was at least as important as its democratic nature, but this subject is ignored. The index is embarrassing. The text credits Mary Douglas, the designer of the enclave model, several times with key contributions, but she does not appear in the index. Key matters discussed again and again, such as migration, public policy, and civil society, are absent in the index. Jacob had to labor another seven years to get the prize he really wanted; the project needs less time to complete its task.

**Democratic Reform and the Position of Women in Transitional Economies.** Edited by Valentine M. Moghadam. Oxford: Clarendon Press, 1994. 366p. $60.00.

Barbara Jancar-Webster, *SUNY–Brockport*

*Democratic Reform and the Position of Women in Transitional Economies* is the product of a conference entitled "Perestroika, the 1989 Revolutions and Women," organized by the World Institute for Development Economics Research (WIDER) of the United Nations University (UNU) in Helsinki, Finland, in September 1991. Although the book clearly suffers from the short preparation time, its great merit is its setting of the impact upon women of the first two years of democratic and economic reform in historical and theoretical context. The elaboration and critique of this context is fascinating and renders this book must reading for anyone interested in understanding the male character of the transition process and the importance of gender as a political concept. Objections of political science purists notwithstanding, the book exemplifies the steady and, to this writer's mind, necessary blurring of the fine line between research and advocacy.

The book begins with its conclusion. Women have become invisible in the new political structures of the transition countries. Gender bias plays a major role in the deteriorating economic and political situation for women throughout the region, where highly educated women may be found working in low-skilled jobs simply to survive. Economic reform is a masculine project.

If this conclusion were all the book were about, the reader would do well to yawn and set it aside. But the authors of this edited volume go farther. Why, they ask, has the transition hit women adversely? The reader is given several answers from different viewpoints and left to form her own explanation from the discussion. The book is divided into three parts. The first addresses the problem of democratization and its impact on women in East Central Europe, the second looks at the impact of *perestroika* and economic reform in Russia, and the third examines case studies of countries to assess the impact of economic reform upon women's employment.

In the first section, Wolchik traces women's current exclusion from political power to the uneven pace of change in women's different roles under communism. Although women moved fairly rapidly into the highest levels of education, job feminization replaced women's traditional economic roles, while communist patriarchy thrust the main burden of childrearing upon women. In the same section, Einhorn argues that the very rights the communist regimes gave to women, such as the right to stay home and care for a sick child, have now become the reasons for the dismissal of women from the work force in the reforming economies. Rueshemayer stresses the loss of social benefits and high unemployment as the main explanation for women's exclusion from decision making. Governments at all administrative levels today are without the resources to sustain adequate social welfare programs. These economic and social factors are supplemented by the persistence of traditional attitudes toward female roles among women. Many women have tended to view the new and unstable conditions as a pretext to retreat to the home for survival. Men want them there, there is no psychological conflict, and the children need them. The conclusion of the section is that the old "double-burden" so talked about during socialism continues to bias politics against women and hamper their advance. On the one hand, the democratization process has increased formal rights and created a space for women to organize independently. On the other, the logic of economic reform argues against the maintenance of those support systems developed under socialism that would enable women to take advantage of their new-found rights.

In section two, Lane supports the relevance of the loss of social welfare support to women's declining status in the new Russia. Bodrova, on her part, highlights the persistence of traditional attitudes. In her analysis of three nationwide polls on women's status undertaken during *perestroika*, she sees a significant dichotomy in women's approach to work and home. She finds Russian women's solutions to problems in their professional and social life dominated by the egalitarian approach. By contrast, the more the issues touch on women's role in the family, the more strongly do patriarchal attitudes reassert themselves. Kauppinen-Toropainen provides substantiation for this thesis. In her study of work satisfaction in Estonia, Moscow, and Scandinavia, she found working life in the former Soviet Union to be more sex-stereotyped than in the West, even though women had better access to male-dominated occupations.

In the final section, four case studies stress the links among loss of social benefits, the high level of educational attainment of women during the socialist period, and the high unemployment of women during the transition period. Paukert of the International Labor Office found that many women in Slovakia were unaware that women's unemployment was greater than men's and that this trend would continue. Fong and Paull give a vivid statistical representation of the precipitous drop in women's economic status that occurred after 1989. All the studies stress that the communist feminization of labor, with women predominating in the textile and service industries, had left these sectors the most vulnerable to market rationalization after 1990. In her concluding chapter, Moghadam draws fascinating parallels between restructuring in the former communist countries and the experience of the Asian NICS. Her most pertinent discussion analyzes the relative cost of female labor in the two regions. Communism's many social benefits for women made female labor increasingly expensive, she argues. With few or no mandated benefits, women's labor in the NICS remained cheap. In Moghadam's view, restructuring of the former communist countries inevitably entails bringing the cost of female production and reproduction down to the global price. This forced downsizing cannot be expected to alter definitions of gender or effect a decline in pretransition patriarchal attitudes. Modifications in patriarchy have taken place at the global level only because women's cheap labor costs brought a rapid increase in the number of wórking women worldwide with a resultant integration of women into national and international political and economic life.

Moghadam's final assessment constitutes a nice fusion of the objectivity of research with the subjectivity of advocacy. Given the current internationalization of the global labor market, the new "flexibility" in employment, and the sheer numbers of immigrants ready to work, East European and Russian women may not have much opportunity to reverse the downward trend in their situation. Moghadam sees no reversal of women's declining status without the development of a well-organized feminist movement.

This reviewer is delighted to find her earlier position confirmed and advocated. As a participant at the Fourth World Conference on Women in Beijing, she was most impressed by the organizational strength and ability of women from Asia and Africa. So successful were the communist regimes in obscuring the need for and prohibiting the formation of grass-roots groups that women today in the former communist countries consider feminism as either a dirty word or not applicable to their situation. The collapse of communism revealed the fallacy behind the myth of the all-powerful benevolent state. Gender *is* one of the most important aspects of gobal politics. Perhaps the most significant message from *Democratic Reform* is that in cases where women have been perceived as the objects of politics for too long, when political change eventually comes, they lack the requisite skills to do battle for themselves.

**The Rebirth of Russian Democracy: An Interpretation of Political Culture.** By Nicolai N. Petro. Cambridge, MA: Harvard University Press, 1995. 225p. $39.95.

Archie Brown, *Oxford University*

However questionable many of Nicolai Petro's assumptions and his account of Russian political culture are, the author has made a useful contribution to the history of Russian political thought. He discusses "alternative traditions" (i.e., to those that favor some form of authoritarian rule) in Russian political thought from the eighteenth century to the present day. Petro also provides a concise history of organized dissent in Russia and of the emergence of the rudiments of a civil society in the second half of the 1980s. He is not afraid to set out his own value judgments and is an advocate of an "enlightened patriotism" in contemporary Russia (p. 173), which he sees as gaining ground. He takes an optimistic view of today's Russian communists (even though they are the remnants of the most reactionary wing of the Communist Party of the Soviet Union) and holds that a gradual "social-democratization" of the Communist Party of the Russian Federation now "seems irreversible."

For a reviewer writing between the elections for the State Duma of December 1995 and the presidential election scheduled for June 1996, the situation appears less clear-cut. The electoral process is already far from democratic, with Boris Yeltsin and the "party of power" securing an overwhelming bias in their favor on the part of all national television and most of the press and with the communists threatening to curb the freedom of the mass media even further. It is almost certainly true that even the Communist Party would have to accept a mixed economy and could scarcely go back to the world of five-year-plans, but in terms of their attitude toward authoritarianism they remain far removed from the former communists who have returned to power in social-democratic colors in Eastern Europe. Survey research from the mid-1990s also shows communist voters to be the most nationalistic segment of the Russian electorate (more nationalistic, for example, than a majority of those who vote for Zhirinovsky).

Many of Petro's generalizations raise serious doubts, beginning with the title of his book. Russia has had its share of democratic thinkers and people with democratic aspirations. It has also had periods of evolving political pluralism, among them the last decades of the Russian Empire and the period since 1987. But it is far from clear when the political system, or the country as a whole, could reasonably be called democratic. Even today the absence or fragility of many of the fundamentals of democracy—such as agreement on the rules of the game, a rule of law, accountability of politicians, and free and honestly conducted elections—hardly bear out Petro's optimism.

The author's generalization that "the failure of Sovietology is rooted in its infatuation with political culture" (p. 4) is questionable on two counts. Most of the current condemnation of Western specialists on the grounds that they did not predict the year in which the Soviet system would come to an end is plain silly. Even the mayor of St. Petersburg, Anatoly Sobchak, spoke on 14 August 1991 about the "historic event" that was to occur later that month—the signing of a new voluntary Union Treaty that he hoped would be "as long-lived as the American Declaration of Independence, and serve as the same reliable political and legal basis of the renovated Union." In fact, the Union Treaty was never signed, for the hard-line putschists launched their attempted coup specifically to prevent Gorbachev from signing it and thereby achieved the opposite of what they intended, hastening the collapse of the Soviet Union. But from at least 1990 many Western specialists on the former USSR took seriously the prospect of the breakup of that Union and were not one whit more myopic in this respect than the Russians themselves.

It is also simply untrue that "Sovietology" as a whole was "infatuated" with political culture. There was an enormous diversity of view and of approach within this field of political science and only a small minority of its practitioners devoted much time to the study of political culture—hardly surprising, since reliable survey data (which many, though not all, scholars would regard as a sine qua non of research on political culture) were unavailable at any time earlier than the late 1980s. In the absence of professionally conducted survey research in conditions in which respondents were not afraid to answer honestly, a number of authors drew upon a variety of other sources in order to offer their own interpretations of continuity and change in Russian political culture. Their choice of evidence, Petro complains (p. 15), was "subjective." Indeed, it was, but it was no more subjective than Petro's own selection of facts and emphases in his study of Russian history to bolster his own interpretation of Russian political culture.

Some of his interpretations of processes and personalities are bizarre. *Perestroika*, the author claims, is "perhaps best understood as the last official attempt to resurrect National Bolshevism" (p. 59). Today, however, it is criticized in Russia, inter alia, for having been too idealistically internationalist. Gorbachev, Shevardnadze, and Yakovlev are condemned for not having defended vigorously enough Russian national interests. That may be unfair, but it is not so far from the truth as the notion that they were "National Bolsheviks." On the same page Petro refers to "Gorbachev's perplexing commitment to the ideals of socialism." This misses the point that during the *perestroika* years Gorbachev changed fundamentally what he meant by socialism. "Neo-Bolshevism" became for him a term of abuse, and he moved from being a socialist of a relatively orthodox Soviet communist type to a socialist of a social-democratic type. That shift was crucial and brought Gorbachev closer to West European social democrats such as Felipe Gonzalez than to the kind of people who are in charge of the Communist Party of the Russian Federation today.

Petro gets Gorbachev wrong in more ways than one. He states (p. 148) that "Gorbachev was *never* able to place himself ahead of political developments in society because he did not understand how fundamentally different the political aspirations of Russia's alternative political culture were from his own." But in 1988 there was no demand from below for contested political elections, and it was Gorbachev who pushed the decision to move to such elections through a reluctant party conference. Much of what Petro applauds followed from that, yet the initial thrust toward democratization of the political system in 1988 came not from mass pressure but from a minority within the political elite (who, decisively, included the party general secretary). Similarly, Petro refers (p. 158) to "a fatal lack of popular support for [Gorbachev's] policies," but that is also inaccurate as a generalization. Gorbachev was in power for less than seven years and for his first five years was the most popular politician in Russia and the USSR. According to the survey data of the All-Union (now All-Russian) Centre for the Study of Public Opinion, headed at that time by Tatyana Zaslavskaya (with its present Director Yury Levada already a principal researcher), it was as late as May 1990 that Boris Yeltsin overtook Gorbachev in popular esteem.

Petro's rather forlorn battle against "subjectivity" is not helped by his own peculiarly selective use of evidence. Thus, he cites a series of social indicators (pp. 99–100) on health, alcoholism, education, and the social infrastructure in the late 1980s without mentioning that in all these respects things have become worse in post-Soviet Russia. His conviction that Russian society "would not willingly embrace socialism" may be true or false, depending on how one chooses to define socialism. But, on one rather traditional criterion—namely, attitude to a state-planned as against a market economy—in every year since 1991 support for a market economy has dropped and only among those under the age of thirty is there a majority in favor it.

Data that are inconvenient for Petro's rather optimistic account of the development of Russian political culture tend to be overlooked. Most that is of value in this book concerns the variety of traditions within Russian political thought. As an overall interpretation of Russian political culture, it is, however, at least as deficient as the earlier work in that field that the author criticizes.

**Mexico Faces the 21st Century.** Edited by Donald E. Schulz and Edward J. Williams. Westport, CT: Praeger, 1995. 218p. $59.95 cloth, $18.95 paper.

Joseph L. Klesner, *Kenyon College*

Schulz and Williams have put together an edited volume of essays about contemporary Mexican politics that is neither the best nor the worst of several such edited works that have recently been published. The title suggests that the volume will address Mexican affairs at the end of the twentieth century, during the administration of Ernesto Zedillo (1994–2000). For the most part, the authors, who originally produced these essays for a conference at the University of Arizona in 1993, do try to tell the future, based of course on the past. The experience of the administration of Carlos Salinas de Gortari (1988–94)—both the profound changes brought to Mexico by Salinas and the political and economic difficulties attending his transfer of power to Zedillo—looms large in this work, as it should. The editors, particularly Schulz, do an admirable job in trying to update the work of the individual authors to take account of events at the end of 1994 and into 1995.

Edited volumes can be idiosyncratic, depending greatly on whose work the editors bring together to produce the overall work. This book is no exception. Compared to similar works, this volume gives greater emphasis to the military, to U.S.-Mexican affairs and NAFTA- and border-related matters, and to selected public policy issues, again especially as related to U.S.-Mexican relations. Although Roderic Camp provides a good overview of the progress of democratization, electoral politics and political party development receive far too little attention in this book, considering the increasing importance of each. Even the governing Institutional Revolutionary Party (PRI) receives relatively little focused discussion, while the opposition National Action Party (PAN), which has won most gubernatorial elections and several important mayoral elections since Zedillo came to power, and the Democratic Revolutionary Party (PRD), which suffered severely under Salinas, get very little notice. Maria Lorena Cook contributes a fine chapter on state-labor relations; other interest groups, however, receive less-focused study, including both the peasantry, whose situation is very volatile after the neoliberal reforms have begun having their effects, and the business community, sectors of which benefited handsomely under Salinas, other parts of which have suffered under stiffened foreign competition. Isidrio Sepúlveda describes the erosion of corporatism in the face of the liberalizing policies of Salinas and the explosion of new civil associations that will foster the growth of civil society, but provides only a superficial account of the florescence of popular organizations in Mexico. In short, the politics of parties, interest groups, and civic associations receives much less attention than warranted in a volume with this title, especially since this politics is particularly important because democratization in Mexico will have to involve important institution-building in these informal organizations, not to speak of the formal institutions.

Readers will find this volume quite useful, however, for its discussions of civil-military relations and of U.S.-Mexican affairs. Civil-military affairs have traditionally been ignored among students of Mexican politics, though the recent work by Camp on *Generals in the Palacio: The Military in Modern Mexico* (1992) is an important exception. Stephen Wager describes how modernization in the structure of the armed forces is likely to make it better able to confront its two major future tasks, containing violent unrest provoked by the economic disruption associated with neoliberal policies and counternarcotics efforts. Wager and Schulz discuss the implications of the Chiapas rebellion for civil-military relations. Clearly, the military is under much greater pressure than it has been in decades from both the political elite and the public because of its greater use in internal security affairs and because of the declining authority of the PRI and the presidency. Of course, keeping the military in the barracks except for when the army is deployed remains a priority of Mexico's civilian leaders, but that may grow increasingly difficult as the military is called on to take action against Mexican citizens.

Mexico's economic dependency on the United States has grown over the past decade, as has the political vulnerability of the Mexican leadership vis-à-vis Washington. As Schulz and Al Pérez argue in separate contributions, although Washington may advocate democracy in most of the world (at least rhetorically), in Mexico political stability is a higher priority of U.S. foreign policy. Pérez, a career U.S. Foreign Service officer, considers NAFTA's greatest contribution to be in defining a new economic relationship with Latin America, anchored in the U.S.-Mexican agreement. Democracy is at best a side issue here.

Polishing their essays during what the editors label "The Year of Living Dangerously"—that is, 1994, with the Chiapas rebellion, political assassinations, and the disastrous devaluation at the end of the year—the authors' tone is mostly somber. Schulz's concluding essay is the best in the book. In it he portrays Zedillo as a weak president increasingly unable to promote democratic reforms, but whose weakness is as likely to produce political breakdown as democratization. He describes the grave economic difficulties now faced by Mexicans, particularly by those of the lower classes who have suffered under economic restructuring for over a decade and who suffer even more under the current economic recovery plan. And he describes how Mexico's growing economic dependency on the United States threatens to produce a backlash of economic nationalism in Mexico even as it puts greater pressure on the U.S. government to attend to Mexico's economic needs, hardly a vote-winning policy among the U.S. public. Whether Mexico faces a future this bleak only time will tell. This volume does provide a healthy dose of realism. Moreover, one need not be a political scientist or a Mexicanist to read easily and understand the issues under discussion here; the editors have done an admirable job in making the work accessible to the layperson.

**Immigrants and Nationalists: Ethnic Conflict and Accommodation in Catalonia, the Basque Country, Latvia, and Estonia.** By Gershon Shafir. Albany: State University of New York Press, 1995. 279p. $19.95 paper.

Milton J. Esman, *Cornell University*

The phenomena the author examines and seeks to explain are the migration to industrialized regions by culturally diverse citizens of the same state and variations in their reception by the indigenous society. Migrants are believed to be generally willing to integrate into their host societies. He identifies two expressions of nationalism: "hegemonic nationalism," which is likely to be espoused by self-confident modernizing elites who welcome an expanded labor force, and "corporate nationalism," the defensive strategy of elites who fear modernization and immigration as threats to their economic interests and their privileged way of life. Hegemonic nationalism results in policies that encourage and welcome the structural assimilation of immigrants. By contrast, corporate nationalism fosters policies of exclusion and institutionalized discrimination. (A third option, "multiculturalism," will be discussed later in this review.) The familiar civic and ethnic bases of citizenship are associated respectively with hegemonic and corporate nationalism. The reception of immigrants is assumed to be determined by elite initiative.

To confirm these relationships empirically, the author examines four contemporary cases: Catalonia and the Basque country in Spain, and Latvia and Estonia during the transition from Soviet control to national independence. Since each of these regions was more advanced economically than the rest of its country, they attracted large numbers of migrants; but the reception of these migrants differed markedly. The Catalan bourgeoisie, in firm control of their dynamic economic enterprises, promoted structural integration and cultural assimilation. The Basque elites have been divided between modernizing urban industrialists and small town–rural notables; the latter, fearing modernization, promoted harsh, exclusionist policies toward migrants. The Latvian and Estonian elites exerted no control over Soviet-sponsored economic enterprises and military installations that attracted large numbers of Slavic, mostly Russian, immigrants; predictably, they adopted exclusionist and discriminatory policies when they finally regained their national independence. During the Soviet occupation, most Russian immigrants, except for dissident intellectuals, identified with the dominant power and its more prestigious culture. But, according to the author, the Russians opted for inclusion in the newly independent Baltic states rather than the status of national minorities.

Both hegemonic and corporate nationalism are analytical categories; in reality they are variable, with combinations of the two being adopted at particular times. Here is where multiculturalism comes in. This alternative to assimilation and exclusion or subordination accepts the equitable incorporation of immigrants into the political and economic structures of society while providing opportunities for cultural maintenance in autonomous institutions, thus presenting less of a threat to indigenous political elites than the power-sharing status of national minorities. The author, who advocates this version of minority rights for Russians in Latvia and Estonia, seems unaware of the mostly symbolic function of multiculturalism as a way station to full assimilation in countries, such as Canada and Australia, that have formally adopted it.

Like many social scientists who attempt to address political phenomena such as ethnic conflict, the author interprets them as derivative of deeper forces, in this case economic modernization. He holds the general proposition that nationalism in its various forms is sponsored and manipulated by elites to serve their material interests or psychological anxieties. All political phenomena can presumably be traced to elite initiative. A comparative assessment might ask, however, whether the fierce anti-immigrant agitation in Germany originated in that country's economic or political elites, or whether the Le Pen-Front National movement in France reflects populist, rather than elitist, sentiment. These anti-immigrant pressures were, in fact, deplored in elite circles in these countries; they responded belatedly only to maintain their electoral constituencies.

Comparing Spain and the Baltics, the ambitious rationale for this project, strains the comparative method. The industries that attracted migrants were established and operated in the Baltics not by indigenous capitalists, as in the Spanish regions, but by the hated Soviet state. The migrants included senior managers and professionals as well as rank-and-file workers. All were privileged as members of a colonial caste that worked and lived exclusively in the Russian language. Once the Baltics regained independence, the local Russians continued to control these economic assets and were perceived as irredentist threats to independence. The indigenous society in Catalonia and the Basque country had no similar reasons to hate and fear the migrants. In all cases immigration was a common factor, but the underlying structures of power were so different as to compromise meaningful comparison.

Readers interested in ethnic politics and the political consequences of migration can profit from the author's carefully documented treatment of internal migration in Spain and the current predicament of Russians in the "near abroad" following the withdrawal of their Soviet patrons. Some may find the elite-based theory that the author constructs to explain these developments to be overdetermined and his espousal of multiculturalism a questionable prescription for deeply divided societies such as the Baltics.

**Deng Xiaoping: Portrait of a Chinese Statesman.** Edited by David Shambaugh. Oxford and New York: Oxford University Press, 1995. 172p. $16.95 paper.

Quansheng Zhao, *American University*

Contemporary Chinese politics can be divided primarily into two periods: the era of Mao Zedong from 1949 to 1976 and the era of Deng Xiaoping from 1978 to the present (with a transitional interval from 1976 to 1978). Any thorough examination of Mao and Deng, the two paramount Communist leaders, will enhance our understanding of post-1949 Chinese politics. A year after the death of Mao, Dick Wilson (1977) edited a book entitled *Mao Tse-tung in the Scales of History: A Preliminary Assessment*, evaluating Mao's legacies in contemporary China and world affairs. Now with Deng in his 90s and China moving toward the post-Deng era, the book under review will undoubtedly make a timely and welcome contribution not only to our comprehension of Deng Xiaoping as an individual leader but also China's development in the most recent (and still current) era, the era of Deng, and will remain useful to our research and teaching of China and to the field of comparative politics.

Initially published in the *China Quarterly* as a group of essays, this book consists of seven articles and an introduction written by well-known sinologists. The division of labor among the authors in their examinations of Deng Xiaoping are: the political culture perspective (Lucian Pye), Deng's biography in the pre-1949 period (Benjamin Yang), the political dimension (David Shambaugh), economic affairs (Barry Naughton), social reforms (Martin Whyte), the military dimension (June Dreyer), and foreign policy issues (Michael Yahuda).

Although they examine different subject areas, there are at least three common elements among these articles. First, in order to make an overall assessment of Deng Xiaoping's career since he became China's paramount leader in 1978, each chapter more or less traces back to Deng's early experience in the Mao era or even to the pre-1949 period. Several authors also discuss Deng's personal character. For example, in his study of Deng's unique role in contemporary Chinese politics, renowned China watcher Lucian Pye presents a powerful and penetrating analysis of Deng from the perspective of political culture and political psychology, thereby advancing our knowledge of Deng as a leader and his decision-making style. Benjamin Yang also provides a useful and detailed account of Deng's experience in the pre-1949 period.

The next common point is that virtually every chapter in the book makes some comparisons between Deng and Mao, providing interesting observations. While both Mao and Deng are Communists with Chinese characteristics, there are clear contrasts between the two in terms of how to rule China. Mao emphasized political campaigns leading to China's "repeated economic disasters," whereas Deng has promoted economic modernization and "presided over China's economic revival" (Naughton, p. 105). Quite different from the leadership style in the era of Mao, Deng "did not rely on coercion, charisma or ideology, but preferred to rule through formal Party institutions and Leninist norms" (Shambaugh, p. 2). In addition, in his social and administrative reforms, Deng has put more emphasis on meritocratic criteria in China's personnel policy (Whyte, p. 115).

Third, Deng's working style is another central attention of the contributors. The detached, pragmatic, and unemotional manner of Deng Xiaoping in dealing with important policy issues, together with his straightforward, uncomplicated, and brisk style of decision making, was once described by Zhou Enlai as that of *juzhong ruoqing*—"lifting something heavy as if it were light" (Pye, p. 28). For example, Deng did not have "a clear blueprint" for his economic reform (Naughton, p. 102), and his role in the military was, at least publicly, "invisible" (Dreyer, p. 135). This noteworthy leadership style, therefore, may well have helped to facilitate Deng's relative success in implementing China's open and reform policies since 1978. As Naughton points out, " 'Not to engage in debates' about unresolvable principles is in fact one of Deng's guiding principles" (p. 99).

There are also differences among the contributors in their overall evaluations of Deng. On the one hand, Deng is praised for his accomplishments in various areas: Deng's "economic legacy is overwhelmingly positive and quite secure" (Naughton, p. 83); his "military successes" will be remembered (Dreyer, p. 142); and his historical claims "must rest on his reinterpretation of China's national interest and on engaging his country once again with the rest of the world" (Yahuda, p. 163). On the other hand, Deng is criticized for his failure in political and social reforms. He did not bring "political freedom and security" to the Chinese people (Pye, p. 35) and "failed to create the institutional mechanisms for improved political participation" (Shambaugh, p. 82). This mixed picture of Deng suggests that he might be assessed differently when considering different dimensions, such as politics versus economy. There is, therefore, a need to bridge these dimensions, examining the mutual impact between China's economic reforms and its political development. One such effort in this direction was presented in Susan Shirk's (1993) *The Political Logic of Economic Reform in China*.

One of this book's few shortcomings is the inconsistency of some basic information about Deng. There are, for example, different answers to a key question—When and where did Deng join the Chinese Communist Party (CCP)? Yang argues that it was "most likely" in Moscow in 1926 (p. 39), whereas Shambaugh claims that it was in 1924 in France (p. 54). This is an important matter: Among the first generation of revolutionaries seniority of party membership was a key to determining one's position within the party's hierarchy. To clarify this matter, I checked three accounts of Deng's life—Uli Franz's (1988) *Deng Xiaoping*, Richard Evans's (1993) *Deng Xiaoping and the Making of Modern China*, and Deng Maomao's (Chinese edition, 1993; English edition, 1995) *My Father Deng Xiaoping*. These three authors confirmed that 1924 was the year when Deng joined the CCP. Deng Maomao, Deng's daughter, provides the most detailed explanation for the above-mentioned confusion. According to her record (pp. 80–7), Deng attended the Fifth Congress of the Chinese Communist Youth League (CCYL) in Europe in July 1924 and was elected one of the three members of the League's Executive Committee (secretariat). Since there was a party regulation that anyone holding a leading position on the CCYL's Executive Committee would automatically become a member of the CCP's European Branch, Deng's party membership indeed began in 1924. Since this book is a significant publication and people are likely to refer to it in the future, it is important to provide precise answers to critical questions so that unnecessary confusion can be avoided.

There are also a few misspellings of the names of influential Chinese figures. For example, Chen Yun, Deng's long-time colleague and chief rival in the 1980s and early 1990s, is spelled *Chun Yun* (pp. 32–3); Deng's wife Zhuo Lin becomes *Zhou Lin* (p. 19); and Zhao Shiyan, the founding secretary of the CCYL in Europe, becomes *Zhao Shiyun* (p. 21). However, these minor problems do not negate the book's contribution to the field.

As this is a collective volume with perspectives from a variety of fields, including political science, economics, sociology, and history, it is understandable that there is no overarching theoretical framework spanning the book. To get a more general picture of China's elite politics, one may wish to read another group of articles published in the *China Journal* (July 1995), with contributions from such leading students of China as Lowell Dittmer, Lucian Pye, Frederick Teiwes, Tsou Tang, and Andrew Nathan (with Kellee Tsai). This recent collective effort develops theoretical models and conceptual guidance for the study of Chinese politics in general and elite politics in particular. It will be a considerable enjoyment for people interested in Deng and China's elite politics to read these two groups of essays together.

By offering "careful, comprehensive analysis and reasoned judgments" of Deng Xiaoping (Shambaugh, p. 3), this volume has set high standards for future studies. It is almost certain that more research on Deng will be conducted in the time to come. Another meaningful way to evaluate Deng's life and times is to examine him in a comparative perspective. One may, for example, compare Deng with Mikhail Gorbachev of the former Soviet Union to see how the leaders of the two communist giants reacted differently to the economic and political demands of their respective peoples with strikingly different consequences. Minxin Pei's (1994) *From Reform to Revolution: The Demise of Communism in China and the Soviet Union* is a noticeable effort in this direction. One may also explore similarities and differences between Deng and the late Chiang Ching-kuo—a classmate of Deng in the 1920s when both studied in Moscow and the leader of Taiwan from the 1970s to the late 1980s—who provided effective leadership for Taiwan's economic modernization and initiated the process of democratization in Taiwan.

**The Global Expansion of Judicial Power.** Edited by C. Neal Tate and Torbjörn Vallinder. New York: New York University Press, 1995. 556p. $45.00.

Jerry Waltman, *University of Southern Mississippi*

Not many years ago if you told someone at a political science convention that your interest was comparative judicial politics, it ordinarily prompted a quizzical look. Recently, however, several factors have combined to resurrect interest in courts and legal institutions: the new institutionalism, a renewed interest in natural law, a dissatisfaction with pluralism, and the need for new constitutions in the emerging democracies. In response, there is a growing literature addressing the subject.

This volume is composed of papers presented at a conference organized by the Research Committee on Comparative Judicial Politics, an affiliate of the International Political Science Association. As is so often the case with such collections, the book is too long and, despite some efforts, lacks a theoretical or thematic consistency. However, many of the individual chapters are quite well done, and the best are very good indeed.

Torbjörn Vallinder introduces the book with a theoretical essay arguing that a distinction should be drawn between two types of "judicialization." On the one hand, there could be an increase in the power of the courts vis-à-vis a nation's legislative and executive institutions; on the other, there is the adoption of judicial-like procedures by the other branches of government. The only problem with this chapter is that it is too abbreviated, running only 11 pages, and leaves too many aspects of this intriguing scheme unelaborated. Neal Tate follows with a chapter laying out the possible causes of both types of judicialization throughout the world, and adduces the usual list.

The remaining chapters—23 of them—are a potpourri: Some focus on a single country, some present cross-national research, and some offer policy critiques. A few utilize Vallinder's framework, some mention it in passing, while a number ignore it altogether. Some present adequate background material; others offer little or none. The noteworthy chapters fall into two categories. The first is composed of country overviews and historical narratives. The second is made up of papers that combine rich background and analytical material in insightful ways.

Martin Shapiro provides an engaging overview of the judicialization of politics in the United States. His major point is that pulling the courts into politics will in the end necessarily diminish the distinction between the judiciary and other institutions of government. Part of the power and prestige of courts has traditionally rested on the very differences between them and legislatures and executives, power and prestige that will be sacrificed as more partisan issues end up on judicial dockets. Peter Russell analyzes the impact of the Charter of Rights and Freedoms on the Canadian judiciary. He argues that the courts have actually been quite restrained when called upon to interpret the Charter. The Charter's most important long-run impact may not therefore be a heightened judicialization, but a shift in political attitudes. Specifically, its emphasis on individual rights could well erode the collectivist traditions of both Tory and social democracy. Martin Edelman provides an engaging glimpse into the Israeli situation. Jewish immigration to Palestine was accomplished by various organizations that served state-like functions. After independence, these groupings carried on as political parties, factionalizing almost every aspect of life. The military and the courts emerged as the only institutions that transcended the narrow partisan world most Israelis lived in. In recent years, as political paralysis has engulfed the Knesset, the role of the courts has grown.

Cheryl Thomas writes a useful summary of the effort to establish judicial review in the Soviet Union as it collapsed. William Kitchin examines the role of the judiciary in Russia up to 1992. He stresses that creating an independent judiciary is part of the larger struggle to import Western constitutionalism, an effort that will be tested severely on several fronts. Neal Tate offers a discussion of the attempt to resurrect the power of the Philippine courts in the post-Marcos period.

Five of the chapters are of especial usefulness to students of comparative politics, for they place their analyses of judicial politics within a broader context of political structure and history. Carlo Guarnieri's analysis of Italy's judiciary is a model of what a chapter in a book such as this should be. It comes to the paradoxical conclusion that, while the Italian judiciary is quite independent, that very fact has limited the scope of its political power. This has resulted, he argues, from the attempt to import certain practices from common-law countries without changing the bureaucratic structure of judicial careers. Christine Landfried highlights the interpretive role of the Constitutional Court in Germany and its relation to democratic theory with skill. Jan ten Kate and Peter van Koppen analyze the Dutch case, detailing how the role of the Dutch courts has hinged on the interweaving of domestic and international law. This has led to the rather curious result that the Dutch Supreme Court can declare acts of Parliament void because they violate an international treaty but not because they violate the Dutch constitution.

The two best chapters in the book are Barry Holmström's on Sweden and Nico Steytler's on Namibia. Holmström lays out a threefold classification of judicialization: Vallinder's two plus the degree to which legally trained people make up the legislative and executive branches. He finds that in Sweden the role of the courts has grown, but that the other two facets of judicialization have declined. This stands in sharp contrast to the experience of the United States and the United Kingdom. Steytler's chapter discusses an unusual case. A substantial role for the judiciary was the price SWAPO leaders had to pay for the political pact that ended the civil war. It is purposely designed, therefore, as a counterweight to majoritarian political institutions. The rigidity of the constitution's rights provisions and the courts' role in protecting them may prove, though, to be the judiciary's undoing. For the courts must appease both the white constituency and the other branches of government. With little room to maneuver, this will prove very difficult, perhaps impossible.

This volume convincingly demonstrates that judicialization is an important feature of contemporary political life. But it also demonstrates that it springs from a variety of causes, takes several forms, and may have quite different results from one political system to the next. As comparativists grapple to understand modern politics, therefore, those quizzical looks should disappear.

**Political Culture and Civil Society in Russia and the New States of Eurasia.** Edited by Vladimir Tismaneanu. Armonk, N.Y.: M. E. Sharpe, 1995. 384p. $59.95 cloth, $22.95 paper.

Russell Bova, *Dickinson College*

The role of political culture in the transition to and consolidation of new democratic regimes remains a source of some disagreement among students of democratization in general and among observers of the post-Soviet region in particular. For some, culture is destiny. Applied to the post-Soviet region, this cultural determinism tends to produce varying degrees of pessimism insofar as one might reasonably argue that neither traditional nor Soviet-era political cultures in the region provided much evidence of the kind of "civic culture" on which a democratic regime might be based. For others, however, political culture is more malleable. Such a perspective allows for a greater degree of optimism insofar as economic modernization, external influences, and institutional reform might produce a favorable cultural change.

The various contributors to this book reflect some disagreement on this question of whether political culture is best viewed as cause or effect. Yet, all seem implicitly to accept that the character of the contemporary political culture remains an important indicator of democratic consolidation. Thus, each of the chapters seeks to describe and analyze the emergent political cultures of the region. Russia and Ukraine receive the most attention, though individual chapters on Estonia and Kyrgyzstan and regional assessments of Central Asia and Transcaucasia help round out the volume, which is the seventh in what will eventually be a ten-volume series produced by the Russian Littoral Project under the direction of Karen Dawisha and Bruce Parrott. The essays originated in a series of workshops held in October–November 1994 and thereby reflect developments through the first two or three years of the post-Soviet era.

Among the most interesting of the chapters are the three that are based on survey research. Cynthia Kaplan's study of Estonian political culture, Jeffrey Hahn's analysis of public attitudes in the Russian city of Yaroslavl, and James Gibson's survey of support for democratic institutions in Russia and Ukraine all provide original empirical data on which to base their assessments of democratic prospects in their respective cases. The chapters by Gibson and Hahn are especially noteworthy insofar as they draw cautiously optimistic conclusions that stand out in contrast to the pessimism that pervades both the rest of the volume and the field of post-Soviet studies in general. Although Hahn notes that support for democratic political institutions eroded somewhat between 1991 and 1993, he, like Gibson, sees a significant base of popular support for democratic political institutions reflected in the survey data. Of course, such data are not the last word on the subject. Depending on one's perspective, the survey technique either reveals real changes in the political culture that previously could not be adequately measured or is diverting our attention from deeply rooted culture traditions to ephemeral shifts in the language of political discourse. Both Gibson and Hahn are appropriately sensitive to the limits of their method, but their results, along with those of Kaplan, nonetheless constitute interesting empirical contributions to the debate over the issue.

The rest of the volume is devoted to the more traditional, qualitative assessments of trends in political culture and civil society that scholars of the former USSR have most often produced. For the most part, the picture they paint is fairly bleak and in stark contrast to the guarded optimism of Gibson and Hahn. For example, Henry Huttenbach's view of Transcaucasia is one of a region whose political culture is antidemocratic, antiliberal, and ethnocentric. Critical of those who, in their eagerness to find the basis of a more liberal democratic future, tend to exaggerate the significance of civil society in such cases, Huttenbach suggests that civil society is a European phenomenon whose applicability and significance outside the West may be limited. That view coincides with Patricia Carley's conclusion that the political cultures of the Central Asian states are in many ways "simply unrecognizable to people in Western countries" (p. 315). Likewise, the consensus of the chapters on Ukraine is that the development of a democratic culture and civil society has been arrested by the still unresolved question of what it means to be Ukrainian. Only in the more culturally advanced Western Ukraine does Orest Subtelny find evidence of a significant civil society, thereby lending ammunition to the "culture as destiny" school.

The overall contribution of this volume to existing scholarship can be assessed on two different levels. Viewed as a series of case studies and regional assessments of political culture in the Soviet successor states, the book succeeds reasonably well. Most of the chapters provide a useful summary and analysis of information on the state of political culture and civil society drawn from both Western and local sources. In a few cases, as previously noted, original data are collected and analyzed. As is inevitable in an edited volume, some of the chapters are clearly superior to others, but the overall quality is better than average for a book of this type. One might quibble with the cases chosen for inclusion. Russia, Ukraine, and Central Asia as a region are well represented, but other cases are ignored in the process. Even accepting the editor's reasonable insistence on the central significance of the Russian case to the future of all the states in the region, there remains cause to wonder why there is a chapter on Estonia but none on Lithuania, Latvia, or Moldova? Why is there an individual chapter on Kyrgyzstan but only group assessment of the rest of Central Asia?

For those looking for a larger contribution to an understanding of the relationship between political culture and democratic institutions the book will be somewhat less satisfying. To be fair, that was not the primary purpose of the book, and at this early stage in the study of post-Soviet states it may be enough simply to help clarify the confusing situation in that part of the world without worrying about the larger theoretical implications. Yet, at least some of the chapters do explicitly raise larger theoretical questions that the book as a whole does not answer. The mix of concerns that the various chapters address under the general heading of political culture and civil society and the variety of approaches employed to those ends do not allow for making direct comparisons or drawing overall conclusions. For example, some of the chapters suggest the malleability of political culture, but others emphasize the obstacles to change inherent therein. While the individual contributors often have something to say on this issue, the book as a whole does not. Thus, the major audience for this book will be less the theoretically minded political scientist than the area specialist interested in the post-Soviet region or, more specifically, in one or more of the cases examined within that area.

**Indigenous Peoples and Democracy in Latin America.** Edited by Donna Lee Van Cott. New York: St. Martin's, 1994. 274p. $45.00 cloth, $18.95 paper.

Eric Selbin, *Southwestern University*

This book is an important and valuable contribution to both the increasingly crowded literature on democratization in Latin America (and elsewhere) and the rapidly expanding literature on indigenous peoples around the world. Specifically, this text sets out to explain the particular characteristics of indigenous people in Latin America—a far from monolithic entity, as the book reminds us—and their relationship with their societies, their governments, and with each other as democracy is, at least nominally, embraced throughout the region.

Despite a table of contents structured by geography ("The Andean Region," "Mesoamerica," "The Southern Cone"), there are three meaningful sections to this book: a well-articulated overview by editor Van Cott in which she also raises some key points for both scholars and policymakers; an indispensable chapter by Alison Brysk on the rights of indigenous peoples and international politics; and the eight nicely drawn, largely descriptive essays on the region's most important concentrations of indigenous peoples.

Van Cott's introduction enumerates the book's themes, which bind together the case studies: (1) self-determination and challenges to the nation-state; (2) politics and political reform; (3) territorial rights and access to natural resources; and (4) military/police relations with the indigenous population, usually in the context of counterinsurgency and antinarcotics activities. Although she does not identify the relationship as such, these themes are themselves bound together by questions of nation and identity.

Van Cott also astutely raises the "fundamental paradox" at the heart of the relationship between indigenous peoples and the state in the current democratization process: "Besieged state institutions" perceive indigenous organizations "as, at least, an annoyance and, often, a source of instability"; conversely, indigenous groups view "the excluding state itself, not those who demand inclusion in the process of governance," as the threat to democracy (p. 22). Problems of reification aside, the ambiguity here is over what denotes democratization and how democracy is made real. The institutionalization of democracy, democratizing state structures, is proving to be difficult; perhaps more ominously, it would appear to fail at the task of instantiating democracy. What is necessary is the consolidation of democracy, the democratization of the material and ideological conditions of people's daily lives and their resultant commitment to the democratic process—democracy for everybody, everyday, that they believe in.

Van Cott concludes with a critical point that reveals perhaps the chief theoretical omission from this text when she notes the importance of the "*symbolic* challenge" mobilized indigenous groups represent and the degree to which such symbolism resonates with "other actors in society" (p. 22, emphasis in the original). This is a point I will return to below.

Brysk introduces a critical international perspective by providing a connection between the indigenous in Latin America and transnational issue networks. Although couched in terms of international regime theory, Brysk's focus is on transnational issue networks and especially NGOs and social movements. Her invocation of "information politics" (p. 41) is evocative, and she deftly shows how "NGOs are important facilitators for transnational mobilization" and how they are "especially responsive" to social movements (p. 42). Brysk is perspicacious in her assessment that "acting globally has its costs on the domestic front" as "governments have learned to practice international appeasement rather than democratic accountability (p. 45). They too can play the politics of symbolism.

The case studies are authored by an impressive array of scholars and activists, several of whom are members of indigenous communities. While there is some unevenness, the chapters remain largely true to the themes Van Cott lays out in her introduction and as such all make useful contributions. The issues extant here are multifarious, and the process of democratization itself—especially the consolidation of meaningful democratic processes as opposed to the institutionalization of Western democratic procedures—is fraught with contradictions. The attention here to real people in the real world who make real decisions—actors missing all too often from the calculations of social scientists and, with far more dire ramifications, policymakers—is salutary.

Of particular note are Julio Tresierra's chapter on Mexico, useful to anyone interested in understanding Chiapas, and the chapters on the Andean region by Xavier Albó (Bolivia), Jesús Avirama and Rayada Márquez (Colombia), María Isabel Remy (Peru), and Melina Selverston (Ecuador), all of which explore the diversity of the indigenous experiences in that region. Among other valuable insights, these authors remind us that there are important differences among indigenous peoples, even within one country, and that modern state boundaries are often of far greater relevance in capital cities than in the countryside. What cuts across all these cases, as well as those examined by Richard Adams (Guatemala), Carlos Frederico Marés de Souza, Jr. (Brazil), and Esther Prieto (Paraguay), is the importance of symbols and the practice of symbolic politics.

This last point leads to my major complaint with this volume, arguably its major flaw. Despite its overwhelming implicit presence throughout the text and explicit references in virtually every chapter, symbolic politics is not pulled out here as a primary theme. Given that Brysk has recently published a compelling call for the reintroduction of symbolic politics into our analyses—" 'Hearts and Minds': Bringing Symbolic Politics Back In," *Polity*, 1995, p. 4—this seems an unfortunate omission. Understanding nation and identity requires attention to meaning.

There are a few other concerns worth mentioning. Too little attention is paid to the unique perspective of the indigenous who have become part of Latin America's urban melange, a population critical for the democratization process. In addition, the book cries out for a concluding chapter that brings together all the wonderful information and analyses presented. But these are really quibbles.

This is an extremely useful and highly valuable text that should quickly become a mainstay of undergraduate courses in Latin America politics and any graduate courses focused on democratization in Latin American or the policy process of implementing democracy in Latin America (and elsewhere). Everyone would be well served if it was made required reading for policymakers in capitals across the Americas.

**The Legacies of Communism in Eastern Europe.** Edited by Ivan Volgyes and Zoltan Barany. Baltimore: Johns Hopkins University Press, 1995. 338p. $48.00 cloth, $15.95 paper.

Michael Bernhard, *Pennsylvania State University*

This book comprises several extensively researched and well-written chapters that trace political (both domestic and international), economic, environmental, and social developments in the countries of East Central Europe and the Balkans since the collapse of communism. It also includes a number of essays that treat each of these themes from a regional perspective. For this reason, it will serve as a good text to introduce advanced undergraduates and beginning graduate students to the problems of the region.

Despite these strengths, the book is not fully satisfying as a work of political science. The problems begin in the introductory essay by Ivan Volgyes. Noting difficulties in the process of democratization in Eastern Europe ("reemergence of authoritarianism, chaos, intolerance, and even civil and religious wars"), he declares the intention of the authors to determine "to what degree the presence of these attitudes [is] due to values and behavior patterns that were inculcated under the Communist system, rather than those that were prevalent before the Communists took over" (p. 1). Unfortunately, the framework he provides is not well suited to accomplishing this task. In trying to answer the question, he changes the causality outlined in the question above. He makes behavior the dependent variable and explains it in terms of values. From this second perspective, the essence of deciphering the legacy of communism is thus to determine which anti- or ademocratic values are products of the communist era. Then, less than a page later he throws the whole issue into confusion again by seemingly contradicting the premise of his framework by stating that "political behavior does not necessarily reflect ingrained political values" (p. 2).

There are problems with this approach that go beyond those outlined above. First, the framework seems only capable of addressing the problem it seeks to resolve in a most preliminary way. This is because it ignores critical institutional factors in the process of democratization. Values strongly held by a large proportion of the population do not automatically translate into effective political behavior. The control of institutions privileges certain actors and their values. By ignoring the institutional level, Volgyes provides us no way to determine which of the multiplicity of values in postcommunist polities is significant for the success or failure of postcommunist democratization.

Second, it leaves unanswered the critical question of why behaviors that are in some way traceable to communist-era values are more significant for the study of the problems of democratization than other values relevant to that process. Why are they any more important than behaviors that may be characteristic of the process of democratization or typical responses to the introduction of IMF discipline or, as Luan Troxel points out in her essay, the behaviors common to "any modernizing regime after World War II" (p. 227)? Furthermore, are pre-communist values that have somehow survived forty years of communist political socialization any less relevant? Or, for that matter, should such surviving values be considered pre-communist if they had become an enduring part of the population's adaptation to the communist system? These issues are never made clear.

Finally on this point, what purposes would be served in disentangling the values that predated communism from those that were created by it? Volgyes himself admits this difficulty explicitly when he takes up the issue of the economic legacies of communism in his other contribution to the book: "It is not easy to determine the legacies of the Communist system; we really do not know whether, and to what extent, what exists today in the East European economies was caused by the operations or the failures of the system" (p. 48). Later, Troxel points out just how difficult a research task this would be and decides to pursue a reasonable alternative strategy. She selects an important feature of Communist regimes, their hypercentralization, and proceeds to show how this has posed a number of problems for Bulgaria in the current era.

Many of the other authors make similar choices to avoid getting bogged down in the values/behavior framework that Volgyes poses. It is for this reason that the materials assembled here are worth reading. Unfortunately, this means that the chapters do not provide any systematic basis for comparison. While some are primarily concerned with institutional features, others look at behavior, political culture, and even social structure.

Ultimately, I am not convinced that the legacies of communism are those that present the most daunting challenges to the reestablishment of domestic and international stability to Eastern Europe in the postcommunist period. There are long-term regional problems that the communist elites of the postwar period failed to solve, and this failure may be their most daunting legacy. Most accounts of the problems that faced Eastern Europe at the end of World War I list relative economic backwardness; a weak tradition of national independence; societies highly divided or even polarized along ethnic, religious, and class lines; little experience of self-government; and a tradition of prerogative state power. Plus ça change, plus c'est la même chose. Some of the more perceptive essays in the book do capture this. Robin Alison Remington discusses a broad range of short- and long-term problems that caused the disintegration of Yugoslavia. Some of these are direct legacies of Yugoslav Communism; others are simply contemporary to it and some predate it. Bennett Kovrig in his discussion of the historic marginality of Eastern Europe comes closet to capturing the essence of this in his harsh but accurate judgement of the communist era as forty lost years.

**The Brazilian Voter: Mass Politics in Democratic Transition, 1974–1986.** By Kurt von Mettenheim. Pittsburgh: University of Pittsburgh Press, 1995. 289p. $49.95.

Anthony W. Pereira, *New School for Social Research*

Why do people vote the way they do? Specialists in electoral behavior and party systems no longer seem to know. Once-stable patterns of voter identification and party competition are dissolving in many old democracies, while in countries that have recently experienced a transition from authoritarianism, the identifications and propensities of newly enfranchised electorates are often unclear or volatile. Ironically, despite this state of flux, many scholars have been insisting that the consolidation of new democracies is in peril unless voters become loyal to ideologically coherent, disciplined, European-style political parties. Hegel's dictum that the owl of Minerva flies at dusk therefore seems to apply to this field.

Kurt von Mettenheim's book contributes some valuable clarity to the apparent confusion. It applies the literature on voting, party systems, and liberal democracy in the United States and Europe to Brazil, where mass parties developed relatively late by Latin American standards and where party identification among voters has historically been much weaker than in countries such as Argentina, Chile, and Uruguay. Brazil's military regime of 1964–85 was also unusual in the extent to which it allowed electoral competition, albeit of a controlled and limited variety, for many subpresidential political offices.

The book makes three interrelated arguments. First, the military regime of 1964–85 had a depoliticizing effect on voters, but after 1973 increasingly competitive elections caused a rapid realignment of public opinion and voting, resulting in a contemporary electorate that is not as uninformed and apathetic as many specialists have thought. Second, Brazilian voters do not approach elections as their European and U.S. counterparts do. Instead of being influenced by long-term socialization, notions of group interest, or class and ideological identification, Brazilian voters tend to make direct, plebiscitary judgements of executive performance and national issues. Their "immediate and personalistic conceptions of politics are both cause and consequence of the direct plebiscitarian appeals, patronage machines, and the state-centered populist-nationalist tradition which have prevailed in Brazil" since 1930 (p. 5). Third, Brazilian parties are not "underdeveloped," and are more profitably compared to nineteenth-century U.S. parties than to twentieth-century European ones. Instead of lamenting the weak and fleeting character of formal party organizations, their ideological incoherence, and their corruption, scholars should recognize that parties are effective vehicles of state building, policy formation, and mass political integration in Brazil. Furthermore, Brazil's plebiscitary presidential elections organize new party images, inform voters about national issues, and produce clearer perceptions of executive performance than parliamentary elections would. In the author's words, "difference does not imply disfunction" (p. 40).

The book is well-organized though occasionally repetitious. Part I sets out the book's main arguments. Part II (chaps. 2–5) gives an historical analysis of Brazil's four main party systems since 1836; this section is important for Brazilianists but likely to be of interest to comparative voting and party system specialists only for the conclusions the author derives from it. Part III (chaps. 6–10) is the heart of the book, an analysis of pre-election surveys of over three thousand people made between 1974 and 1982 in seven state capitals in the south, southeast, and northeast regions of the country, as well as two rural areas in the southeast.

The book's arguments are generally persuasive. The historical section argues that parties in twentieth-century Brazil, like those in the nineteenth-century United States, have been patronage machines that made plebiscitary appeals and distributed administrative jobs when they won executive offices. In the United States, however, mass parties appeared before state bureaucratization, whereas in Brazil the latter preceded the former. These contrasting historical paths resulted in differences in state-party relations; in Brazil, party leaders have been more dependent on bureaucrats than they were in the United States. The historical section also challenges previous interpretations of Brazil's 1930 revolution as a bourgeois rebellion, a transition from oligarchic liberalism to industrialism, and a military expression of the power of a new middle class, arguing instead that it was primarily a struggle for power brought about by a breakdown of a political pact between powerful state governors and their single-state party machines.

In its analysis of contemporary survey data, the book confirms some conventional perceptions and overturns others. Its argument that Brazilians' votes cannot be predicted on the basis of class and religious attributes or notions of group interest, as in Europe and the United States, is well-supported by the data, and a model of voter choice based on self-placement on a left-right scale, judgment of executive performance, and perception of national issues seems to account for a good portion of variation in voting. Similarly, the survey results reinforce plausible and predictable differences between the minority of voters in rural Brazil, where patronage machines mobilize voters directly and democratic political culture is weak, and the urban parts of the country, where patronage has become less important, indirect appeals via television have increasing impact, and democratic political culture is more firmly rooted. More interesting, the book's survey data seem to suggest that in the mid-1970s, during the most repressive phase of military rule, lower-class voters had considerably more democratic attitudes than did upper-class members of the electorate.

The book's main weaknesses stem from the quality and age of the data, and the sometimes tenuous links between the data and the arguments. As an example of the first problem, readers expecting a thorough treatment of voters and the Brazilian transition will be disappointed to learn that the most recent survey covered by the book is from 1982. Newer survey data on the latter part of the democratic transition that culminated in the 1989 presidential election were therefore not consulted. This was done, perhaps, because newer surveys are not comparable to the older ones; in this case, comprehensiveness was sacrificed for consistency. The choice makes some of Von Mettenheim's conclusions tentative; since the survey results show remarkable changes in the electorate between 1974 and 1982, one wonders if there has not been as much change again since 1982.

Von Mettenheim's argument against what he calls the "civil society empowerment thesis"—the argument that membership in civil associations politicized segments of the mass public during the democratic transition—is an example of a second kind of problem. His treatment of this issue is unsatisfying. The survey data show that members of civic associations such as trade unions, Christian base communities, and neighborhood organizations were no more likely to become involved in election campaigns than non-members. But the author here equates politicization with involvement in election campaigns. This is problematic. The most powerful expressions of civil society's politicization in the 1974–86 period were probably not elections but the movement for an amnesty for political prisoners and exiles in the 1970s, the strike waves of the late 1970s and early 1980s, and the mass demonstrations in favor of direct elections for president that peaked in 1984. Second, the survey does not correlate *active* involvement in associations with participation in election campaigns. This is important, because Brazil's mandatory closed-shop labor laws automatically make all formal-sector workers union members. The data is thus probably too inconclusive to convince theorists of "civil society" and "social capital" that the resurgence of associational life in the Brazil of the late 1970s and early 1980s did not have important politicizing effects.

This is an ambitious and provocative book. Its polemic against liberal denunciations of Brazil's "inchoate" political parties is refreshing but, one suspects, too complacent where liberal theory is too judgmental. However, its insistence that comparative electoral studies must take more account of national differences and refrain from reflexively imposing models derived from European and U.S. experiences is powerful and inspiring. An iconoclastic challenge to existing theory, the book clears the way for a better understanding of changing voter attitudes and choice both within and beyond Brazil.

**Government Survival in Parliamentary Democracies.** By Paul V. Warwick. Cambridge: Cambridge University Press, 1994. 182p. $49.95.

Kaare Strøm, *University of California, San Diego*

Our knowledge of government coalitions in parliamentary democracies used to be much like the proverbial Hollywood films of the 1950s: Much was made of the process of courtship, but little was known about the fate of relationships after the vows had been exchanged. Although we knew intuitively that some partners lived more happily ever after than others, we had little systematic understanding of such differences. And there was no great appreciation of which behaviors caused partnerships to endure or flounder. Needless to say, Hollywood has changed. So, more recently, has the study of cabinet coalitions. Over the past decade or so, we have gained a much keener appreciation of when and why cabinet coalitions survive or fall, and we are beginning to understand how the actions of political leaders bring about one outcome or another.

Clearly, no scholar can singlehandedly take credit for these developments. Yet if a single book should be celebrated as the signal recent contribution to the empirical study of government stability, it would have to be Paul V. Warwick's *Government Survival in Parliamentary Democracies*. To be precise, Warwick's study is without a doubt the single most important work on this topic in twenty years.

Warwick locates his study in the recent concern with constitutional design. He specifically directs our attention to "parliamentarism's darker face": the problem of producing stable and effective governing majorities in fragmented multiparty democracies. The author identifies two competing explanations of cabinet duration: the ideological diversity approach, which stresses the destabilizing effects of intracoalition policy conflict, and the bargaining complexity approach, which highlights (often with the help of cooperative game-theoretic models) the instability resulting from the multiplicity of coalition options. The confrontation between these two perspectives is a main theme of the book.

*Government Survival in Parliamentary Democracies* is commendably compact. The book falls into four parts. In the introductory chapter, Warwick lays out this agenda and reviews the recent literature on party systems and coalition bargaining. Much of his concern here is with the development in the 1980s of the critical events perspective, which questioned the earlier focus on the effects of "structural attributes" on government stability. Some critical event theorists even challenged the idea that systematic causes of government termination or survival could be found at all. Important attempts have since been made to unify these two perspectives, most notably by Gary King and his associates and by Warwick himself. Warwick's first chapter brings the reader swiftly up to date on these developments. The brief chapter that follows is in the main methodological. Clearly and succinctly, Warwick introduces the reader to event history analysis and its application to cabinet coalitions. He then describes the rich data with which he confronts the various models. Here, as elsewhere, Warwick's study is unusually meticulous and comprehensive. The novice reader gets an accessible and fair-minded survey of the various methodological issues. Veterans in the field can follow the minutiae of Warwick's debate with former contributors in his footnotes, in which he doggedly grapples with a host of tricky specification issues, always in a way that demonstrates a close reading of the existing literature and an admirable desire to contribute to the cumulation of knowledge.

Warwick begins his empirical investigation with the variables most classically associated with government survival, government and parliamentary attributes, to which he adds characteristics of the government formation process. Although Warwick adds little to the specification of previous structural attributes models, he tests his hypotheses rigorously and thoroughly. Chapter 4 reveals more of his own agenda: the role of ideology in making or breaking cabinet coalitions. Contrary to much of the recent literature, Warwick argues that ideological diversity, rather than properties such as size or fractionalization, is the most important determinant of cabinet duration.

Whereas the first two empirical chapters examine the effects of static governmental and parliamentary properties, the next two investigate characteristics that vary over the cabinet's lifetime. These include economic conditions, such as unemployment and inflation, but also truly random factors. Warwick seeks to capture the latter through estimates of the baseline hazard rate of government survival—that is, the likelihood that governments will survive from day to day once all systematic causes have been taken into account. It is in these two chapters that Warwick makes his most original contributions, demonstrating that the political impact of economic adversity varies with the left-right location of the governing coalition. Moreover, economic conditions appear to account for the fact that coalitions become increasingly prone to break up as they age.

In the two final chapters, Warwick synthesizes his results with a focus on the destabilizing effects of ideological polarization. Ideological diversity matters; bargaining complexity, as reflected in the availability of attractive coalition alternatives, does not. Warwick argues that these results should lead us to reconsider the ascendancy of game-theoretic explanations of cabinet survival. This argument echoes the critique heard in the early 1970s of the early "policy-blind" models of coalition bargaining. This previous critique did not, however, lead to the abandonment of game-theoretic models, but rather to their improvement. Warwick's challenge may have the same effect. The idea that ideological diversity may be a hindrance to coalition governance seems eminently compatible with much of the recent game-theoretic work in the field. Indeed, it is a key premise in the policy-driven, often spatial, models that have come to dominate this area of research. And the analytical separation of ideological diversity (or policy distance) from bargaining complexity is difficult to sustain in such models. Moreover, Warwick's stress on intraparty and environmental constraints on bargaining should be unobjectionable to many scholars in the game-theoretic tradition. In the end, Warwick's study is probably too inductive to dislodge the game-theoretic approaches.

Whether or not one agrees with Warwick's conclusions, his study will surely have implications for future research on cabinet stability. *Government Survival in Parliamentary Democracies* is an impressive and remarkable book. Its greatest strength lies in the author's dogged, sophisticated, and systematic efforts to get at the true causes of government survival or termination. That is the spirit of mature social science research.

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## International Relations

**Women in the Latin American Development Process.** Edited by Christine E. Bose and Edna Acosta-Bélen. Philadelphia: Temple University Press, 1995. 290p. $49.95 cloth, $18.95 paper.

Rita Jalali, *Michigan State University*

The United Nations International Decade for Women (1975–85) saw the emergence of feminist scholarship on women and development and finally discussion of gender issues ignored by the development literature of the fifties and sixties. Since then there has been a proliferation of literature in this area. *Women in the Latin American Development Process* makes an invaluable contribution to this literature. It provides a feminist perspective on the changing role of women in the political economy of Latin America and the Caribbean. The contributors are some of the leading scholars in the field of women and work. Some of the articles originally appear in a special issue of *Gender and Society*.

The book is divided into two parts. Part 1 examines women's role in the development process, covering topics such as the subordination of women under colonialism, the advantages and disadvantages to women of industrialization, redefinition of women's work, and the role of women in the global restructuring process. Part 2 focuses on Latin American women's daily resistance to their subordinate position at various levels, ranging from the individual and the household to the collective.

Compared to the many other studies that have focused on WID (women in development) issues, this edited volume is noteworthy for its inclusion of a range of views on the impact of the development process on women. Whereas several chapters focus on the negative consequences for women, others—particularly those by Acevedo, Riós, and Rothstein—challenge these negative assumptions. The first three chapters (by Bose and Acosta-Belén, Ward and Pyle, and Acevedo) together provide an excellent overview of the literature focusing on women's labor and its relations to global restructuring.

Two specific case studies from the United States and Puerto Rico illustrate the central role played by women as international trade expands in the global economy. Demand for women's cheap labor has increased both in core capitalist countries and in the export-processing zones of the periphery. For example, data collected by Fernández Kelley and Sassen point to a significantly large presence of women, minorities, and immigrants in the lower echelons of the garment industry and the electronics industry in California and New York. In Puerto Rico, as the chapter by Riós notes, the expansion of manufacturing activities (Operation Bootstrap) also led to an increase in demand for women workers, a development that troubled the Puerto Rican government in a society with a strong patriarchal tradition and a high unemployment rate among men.

Fernández Kelley and Sassen convincingly argue that women's conspicuous presence in the labor force cannot simply be explained by changes in female consciousness; instead it is more a response to the needs of industries to maintain a competitive edge in domestic and international markets through the use of flexible, low-cost labor. As they point out, "Women in advanced and less developed countries are being targeted by employers because they represent one of the most vulnerable segments of the labor force. Women's subordination to men as wives, mothers, and daughters makes them a particularly attractive source of low-cost labor at a time when investors seek to maintain an edge in fiercely competitive international markets" (p. 114).

However, other case studies in the volume indicate that women's increased labor force participation is influenced not only by external economic conditions but also by local gender definitions and kinship patterns. For example, Rothstein's excellent ethnographic study shows that employment of women in San Cosme, Mexico, is seen as a legitimate activity, and family members provide the necessary support. "Through kinship some women, as well as men, are given access to land and other family resources, education, and domestic labor, making their entry into the labor force or commercial sector less vulnerable than it is for women who enter with fewer sources or skills" (p. 186).

Several authors document the harsh conditions Latin American women have suffered as a result of structural adjustment programs instituted by international financial agencies, such as the World Bank and the IMF (see especially the chapter by June Nash).

For Third World women the daily struggle to survive has created the need for collective action. For example, in Uruguay a stagnating economy and high unemployment rates led women to "organize collective meals for the unemployed as well as for those working. They ran health cooperatives, providing care where it had never existed" (Nash, p. 155). In Peru, as scholars elsewhere have also noted, women joined with men in neighborhoods to fight for water, health clinics, wood, kerosene, and schools. Poverty conditions have also forced development agencies to encourage the formation of loan groups (through microenterprise credit programs). Blumberg's lucid, comprehensive analysis of such credit programs in developing countries shows how they empower poor women and men. For women in particular, a greater control over economic resources leads to an increase in self-confidence and greater leverage in fertility decisions and in other household decisions. Their children's well-being also improves.

However, can such struggles over traditional domestic issues really challenge the patriarchal system? Most authors in this volume are convinced it can, for as Safa in a well-written essay observes, "in moving their domestic concerns into the public arena they are redefining the meaning associated with domesticity to include participation and struggle rather than obedience and passivity" (p. 234). Furthermore, according to Chinchilla's insightful essay, Nicaraguan feminists have shown us that it is possible to wage a feminist struggle in developing countries even if the economic conditions do not exist to solve many of the women's problems.

This book makes a valuable contribution to our understanding of women's lives in developing countries. Its focus, however, is mainly on women working in the service and industrial sectors. I would have liked some discussion about women who work in the agriculture sector. Also, some of the authors should have updated their papers to incorporate and reflect upon recent economic changes in Latin America. Yet, these few gaps do not detract from the book's merits. *Women in the Latin American Development Process* is one of the best books to be published recently on women and development. I would recommend this book highly for scholars interested in development issues and women's movements. It should also prove very useful for graduate and advanced undergraduate classes.

**Collective Insecurity: U.S. Defense Policy and the New World Disorder.** By Stephen J. Cimbala. Westport, CT: Greenwood, 1995. 240p. $59.95.

William C. Martel, *Air War College*

The conventional wisdom is that the end of the Cold War leaves much undone in the fields of strategy and defense policy. This gap is particularly dangerous for the United States because, as has been observed by scholars and policymakers alike, the international system has entered a confused and discordant period. This book has chosen the admirable but admittedly difficult task of challenging the conceptual framework for U.S. defense policy in an era of strategic malaise and flux. Many books aspire to address the critical choices for U.S. defense policy, but none has provided answers to the fundamental questions about such policy. There are good reasons why these answers are hard to find, even when the analysis is tightly focused on the more critical questions. Three critical questions are these: Is collective security a cornerstone of U.S. policy? Is Russia America's adversary? Does nuclear proliferation matter?

This book argues that collective security no longer constitutes a guidepost in U.S. policy (p. 125), and thus the United States will be the victim (citing the book's title) of "collective insecurity." The author proceeds in the course of eight chapters to examine discrete factors that influence how the United States defines and conducts defense policy. In Part 1, one chapter conducts a cursory review of American military strategy during the Cold War, while another examines civil-military relations during the Bush administration (even though a closer reading of the chapter suggests that "strategy and defense policy" provides a more apt description of its contents). Part 2 focuses on Russia's role in U.S. policy, notably in chapters that review Russian military doctrine, potential Russian involvement in peacekeeping operations, and the role of nuclear weapons and arms control in U.S.-Russian defense policies. Finally, Part 3 discusses escalation dynamics under the rubric of collective security, challenges posed by nuclear proliferation, and the role of military persuasion in U.S. peacekeeping operations.

The thesis of *Collective Insecurity* is that neither scholars nor policymakers can depend on Cold War thinking to reformulate U.S. defense and security policy. While I am sympathetic to this view, that argument alone does not translate into a coherent framework for U.S. defense policy, much less define the next steps that we must take. Nor does it provide answers to the question of what these changes mean for defense policy.

The fundamental weakness of this book is that, although it defines the problem in a well-written fashion, it fails to provide guidance for future defense policy. The result is that the chapters do not provide direct connections to the argument that U.S. defense policy must change to reflect shifts in the balance of power in Europe and Asia. The book takes on so many diverse issues that it sacrifices coherence. It is not possible, within the limits of parsimony, to examine such a wide range of subjects without raising concerns that disparate papers were integrated into a book on defense policy. Indeed, examining this array of topics skirts perilously close to the charge of being diffuse.

While the chapters focus on issues that the United States must consider as it reshapes defense policy, the author does not integrate those elements into a coherent structure. To illustrate this problem, it would be more useful to give the discussion of collective security more emphasis earlier in the work, given its prominence in the title of the book. While the author is justified to argue that collective security is implicit as the organizing principle for the book, my interpretation is that a broad survey of defense topics makes sense only if collective security no longer provides the framework. Simply put, it is not clear why the author selected these topics.

Another shortcoming is that the book tackles subjects that, while important, have been covered in exhaustive detail before. In this regard, the subject of nuclear weapons comes immediately to mind. I do not believe that it is useful to examine U.S. and Russian nuclear targeting in terms of "post-attack outcomes" with forces on generated alert (pp. 107–8). There are no indications at present that democratic reform in Russia has reached the point of failure where the American people need to contemplate nuclear crises. At the same time, however, there is much in this book that is positive. Other chapters properly illuminate ongoing debates in the literature. The most prominent example is the chapter that organizes the debate about the effects of nuclear proliferation on security through the competing lenses of nuclear positivism, nuclear irrelevancy, and nuclear agnosticism. Indeed, this chapter is a template for how the book should have been organized.

In conclusion, the book's contribution to the scholarship on strategy and policy is to demonstrate that all issues—ranging from fundamental questions about the role of the state to nuclear proliferation, deterrence, and collective security—will remain in a state of intellectual limbo until we move beyond Cold War thinking. This is an extremely valuable point. But having said this, the book does not provide a framework for future defense policy. This weakness diminishes the book's significance to scholarly literature because it fails to articulate a conceptual hierarchy among the competing issues and problems that the future architects of U.S. defense policy must balance. What is left undone is a clear analysis of the foremost themes for defense policy, the next steps for the makers of defense policy, and reflections on what this means for the future. While this book, unfortunately, does not address these issues, it does alert the reader to the complexity of the challenge.

**The Making of Foreign Policy in Russia and the New States of Eurasia.** Edited by Adeed Dawisha and Karen Dawisha. Armonk, NY: M. E. Sharpe, 1995. 374p. $49.95 cloth, $19.95 paper.

**Diplomacy in the Former Soviet Republics.** By James P. Nichol. Westport, CT: Praeger, 1995. 256p. $55.00.

Glenn Chafetz, *University of Memphis*

The purpose of the book edited by Dawisha and Dawisha is clearly contained in the title. The result, however, is less clear. As Adeed Dawisha writes in the Introduction, "the authors were not asked to adhere to a rigid theoretical framework, or 'model,' not least because of the absence of sophisticated theoretical formulations in the study of foreign policy change and restructuring" (p. 5). The absence of a theoretical framework resulted in an eclectic mix of contributions. Some of the authors embrace theory, while others avoid it. Some of the authors present careful atheoretical or descriptive analyses of events, while others prescribe and evaluate policy. Some of the authors mix all these approaches together.

The best chapter is Jonathan Valdez' "Russia, the Near Abroad, and the West." Using the work of Anthony Smith and Donald Horowitz as theoretical framing, Valdez argues that Russia's foreign policy is a function of its search for a national identity. (Other contributors in the volume also rely on the identity concept in their analyses, but they are not as theoretically coherent.) Valdez argues specifically that the domestic popularity of the ultranationalist program and the recent turn to the right in foreign policy are functions of the right's promise to restore value to a Russian identity that has been long suppressed and is currently adrift. Dissatisfaction among the populace results not just from the economic dislocations caused by moving from communism to capitalism, but from a loss of prestige and esteem. The Soviet Union's status as a superpower and Russia's dominance of the USSR made Russians feel good. It is difficult for many of them to accept Russia's decline in status.

The problem with this analysis is that sufficient evidence does not exist to make the leap from the 1993 electoral success of the ultranationalists (as well as the communists who share many of the same foreign policy goals) to the popularity of their foreign policy program. What little polling data that do exist show that the Russian people do not care about foreign policy. Thus, Valdez' claim that "the success of nationalist politicians in the former Soviet Union and Eastern Europe represents more than simply voters' rejection of harsh economic conditions imposed by the transition to a market system" (p. 88) may be true in the domestic realm, but it is not substantiated for foreign policy.

A second problem is that Valdez assumes that the initial Western orientation of Yeltsin's government was merely instrumental (specifically based on expectations of a financial quid pro quo), while the shift toward a more nationalist policy represents something more enduring and deeply felt. This may be a function of the particular conception of identity Valdez uses, but in many identity theories, such as social identity theory, instrumental cooperation can be internalized and become enduring. Moreover, without any data, it is impossible to know with whom the population most closely identified. These are minor cavils, however, in an exemplary contribution.

In addition to Valdez' chapter, there are three other chapters devoted to Russia. The next section addresses one chapter each to the foreign relations of Ukraine, Belarus, and the Baltic states. A third and final section addresses the southern tier with three chapters on the international politics of Central Asia as a whole and one chapter each on Kazakhstan, Azerbaijan, and Armenia. Georgia and, to a lesser extent, Moldova receive mention in chapters on the foreign policies of other states. Karen Dawisha provides a conclusion focusing on the paradoxical symbiotic interdependence of states desperately trying to establish their independence.

The quality of the other contributions varies considerably. All are dated, but that is the nature of the subject matter. Though they do not aim for Valdez' level of theoretical coherence, most chapters competently explain who the relevant individual, institutional, and international actors are in each state's foreign policymaking and why we should care.

The biggest difficulty with the volume is coherence. This is due in part to the absence of a unifying theme, but there are gaps and overlaps in the survey that the editors do not explain. Adeed Dawisha notes in the Introduction: "Much, if not all, of Simonia's analysis is reflected in Jeffrey Checkel's "Structure, Institutions, and Process: Russia's Changing Foreign Policy" (p. 6). Why then include them both? Similarly, no reason is given for devoting four chapters to Central Asia (one entirely to Kazakhstan) and leaving out Georgia and Moldova altogether. Accordingly, this book will be most useful for those interested primarily in Russia and Central Asia.

James P. Nichol's book, though titled similarly, is narrower in scope. It addresses the effects of the diplomatic machinery of the various union republics on Soviet foreign policy and in particular the demise of the Soviet Union. Like much of the work on Soviet nationalities, Nichol focuses on how the multinational character of the Soviet Union contributed to its ultimate collapse. While other analysts have also focused on the inadvertent consequences of the Soviet Union's federal structure in encouraging republican and other local political identities at the expense of the center, Nichol's contribution comes from his examination of the role played by vestigial diplomatic institutions and the experiences of some of the republics.

The book is organized into two sections. In the first section, Nichol details the legal and practical evolution of union republic diplomacy and how that diplomacy changed in the Gorbachev period. The origins of foreign ministries in the union republics followed two paths. Ministries in Ukraine, Georgia, and the Baltic were born during brief periods of independence following the Russian Revolution and in the course of World War II. The others were created quite intentionally by the center for various purposes, including to obtain multiple representation in the United Nations and other bodies (Ukraine and Belarus were founding members), to preserve the fiction of Baltic sovereignty, to create the impression abroad that the Soviet Union was decentralizing and democratizing, to express affinity with Third World regimes, and to encourage international trade and investment. It was the last purpose that motivated Gorbachev to encourage republican foreign policies.

The second section of Nichol's book is a detailed survey of each republic's major foreign policies during the period from 1987, when Gorbachev first encouraged republican foreign connections, until 1991, when the Soviet Union collapsed. Here Nichol describes not only the development of independent foreign policies toward the world at large but also how foreign policy came to accurately characterize the relations among the republics themselves. Nichol argues that these apparatuses accelerated the development of independent foreign policies within the republics as the Soviet Union broke up. There is nothing to object to here. Foreign policy is certainly easier to conduct with institutions in place than having to start from the beginning. The problem is that Nichol ascribes to the independent foreign apparatuses a role in causing the collapse of the Soviet Union. He argues specifically that the discrediting of the Soviet Ministry of Foreign Affairs following its weak opposition to the August coup against Gorbachev "had a major impact, in combination with other coup events, on the decisions of most republics to declare their independence in foreign-policy making and execution" (p. 46). Nichol, however, presents no evidence that supports this claim. Moreover, while a federal system structured along ethnic identities is a permissive cause of secession and disintegration, other cases (e.g., Czechoslovakia and Yugoslavia) did not require republican foreign policy institutions to produce that outcome. The disintegration of the Soviet Union is already overdetermined; another independent variable adds no value.

Despite overreaching in its causal analysis, *Diplomacy in the Former Soviet Republics* is well written and painstakingly researched. It is a valuable addition to both the literature on the late Soviet period and on a little-known aspect of Soviet diplomatic history.

### **Optimal Imperfection? Domestic Uncertainty and Institutions in International Relations.** By George W. Downs and David M. Rocke. Princeton: Princeton University Press, 1995. 159p. $29.95.

T. Clifton Morgan, *Rice University*

Uncertainty is a key feature of many of life's most interesting problems. Choices made by completely and perfectly informed individuals may be difficult—as, for example, in the decision to remove a loved one from life support—but may provide no intrigue or not require much analysis. People faced with a choice from among certain outcomes will simply opt for the outcome that is most preferred. Of far greater interest are those decisions made by people who are uncertain about the consequences of their actions. Uncertainty can arise because our understanding of cause-and-effect relationships is imperfect; that is, we may be unsure of the consequences of our actions. Or it can arise in situations of strategic interaction. If outcomes are determined by the interaction of two or more actors' decisions, and if some of these actors are uncertain regarding the preferences of others, then none may anticipate the precise result of any individual choice. It is this sort of uncertainty that provides the entertainment value of drama, and it is this sort of uncertainty that gives economists, sociologists, and political scientists subjects to study.

Clearly, uncertainty plays a central role in the field of international relations, in which most of the research focuses on determining the conditions of conflict and cooperation among states. In the absence of uncertainty, cooperation is easy to attain and, since the outcome of a conflict can be predicted accurately, there is no point in conducting a conflict through any of the quite costly means we often observe. While it seems fairly obvious that uncertainty makes many of our research problems interesting, few research traditions in international relations address uncertainty directly, and even fewer consider it in a rigorous and systematic fashion. Realists acknowledge that uncertainty is an important factor in the explanation for war, but they focus only on states' uncertainties regarding the balance of power and not on the myriad of other sources of uncertainty facing states in conflict. Many working in the psychological tradition focus on the impact of uncertainty on decision making, but their concern is usually on the psychological impact of uncertainty on cognitive abilities and not on how decision makers can consciously design strategies and institutions to help them cope with uncertainty.

Downs and Rocke focus explicitly on these important questions. They identify a number of specific phenomena that are affected by uncertainty, and they develop rigorous, formal models based on recent advances in game theory to derive their explanations for these phenomena. Specifically, they devote one of their principal chapters to the issue of the diversionary theory of war. The argument that a state leader would resort to violence abroad in an effort to deal with political problems at home hinges critically on the leader's uncertainty regarding whether his actions might threaten his tenure in office as well as on the uncertainty of the electorate regarding the precise motivations for, and necessity of, the leader's actions. Downs and Rocke adopt a principal-agent model to identify the strategies electorates should adopt to ensure that leaders behave as consistently as possible with the electorate's own preferences. They show that such strategies do exist, but they involve the electorate sometimes removing a leader who is performing well, and they can sometimes create an incentive for leaders to "gamble for resurrection" by, for example, continuing a conflict beyond the point that it should prudently stop.

Two chapters are devoted to the analysis of the role of uncertainty in the development of institutions intended to foster international cooperation. Uncertainty regarding the actions of domestic interest groups affects the characteristics of optimal arrangements for the promotion of free trade. Similarly, environmental regimes are hindered by uncertainty regarding the ability of the signatory governments to enforce their international promises at home. Some governments may not have the capacity to force their industries to curtail pollution, for example. It is widely accepted that free trade or environmental regimes face a number of problems in their efforts to monitor compliance with their agreements and to sanction defectors. Downs and Rocke consider a number of strategies that exist to address these issues, such as tit-for-tat, grim trigger, and variations of each. They show that optimal institutional structures must generally allow for some amount of cheating and that the level of uncertainty is a primary determinant of the amount of cheating that must be endured.

Punishments must be designed to minimize defections without threatening the existence of the regime. This may not result in completely free trade or the minimal amount of environmental pollution, but it does allow for greater cooperation and greater benefits than would otherwise be the case. For example, free trade agreements are designed to impose some punishment on cheaters, but the amount of this punishment is not so severe as to destroy the regime. All signatories recognize that at some time they will find that the domestic benefits of cheating will outweigh the costs. If the agreement specified that all actors would immediately revert to noncooperation the moment that any cheating is detected, cooperation would not be sustainable. Similarly, if some governments involved in an environmental regime are not able to enforce the agreement at home, it may be better to tolerate some level of pollution without punishment than to destroy the regime and all the benefits at the first sign of cheating. In each case, the uncertainty inherent in the situation creates a problem for the institutions in terms of maximizing the incentive to cooperate without destroying the basis for cooperation. Downs and Rocke argue convincingly that the creators of international regimes can, and do, find institutional structures that strike an optimal balance between these requirements.

The major weakness of this book is that different models were used for each of the substantive questions. Ideally, we would be left with a single, well-integrated theory that would address the issue of uncertainty in all contexts. This is not a fatal flaw, however. There is a great deal of consistency across models, which all come from the game theoretic tradition and, therefore, share a common set of basic assumptions. The book makes a definite contribution to our understanding. Simply focusing our attention on this area of inquiry makes it worthy of serious consideration, and showing that a disparate set of phenomena share the common feature of uncertainty and that a set of similar models can be used to address these phenomena is certainly useful. The part of the argument I found most compelling was the treatment of institutional structures as endogenous to the overall problem. Institutions do indeed affect the way actors deal with uncertainty about their environment and other actors, but the knowledge that this uncertainty exists and the expectations regarding its impact shape the institutional structures we observe. This is an important argument that will refocus much of our thinking about international phenomena.

**International Organizations and Ethnic Conflict.** Edited by Milton J. Esman and Shibley Telhami. Ithaca: Cornell University Press, 1995. 353p. $47.50 cloth, $17.95 paper.

Manus I. Midlarsky, *Rutgers University*

After nearly three decades of resting in a moribund state, international organization is once again commanding attention in the field of international relations. Reasons for this volte-face are straightforward. The demise of the Cold War effectively ended the one structural regularity of the post-World War II period—namely, bipolarity—that could be counted on to sort the various international conflicts on the East-West dimension. Either one or the other superpower, or under rare circumstances both, generally "handled" the conflict. Thus, there was little opportunity for international organizations such as the United Nations or regional organizations to intervene effectively and independently of great power interests, even for humanitarian purposes. The breakup of empires, both large and small, attended this ending of bipolarity, thereby freeing large numbers of ethnic groups to attempt to regroup as nation-states. Under these circumstances, there clearly is more room to maneuver for explicitly international institutions. Finally, the United States as the sole remaining superpower clearly does not have the resources or inclination to serve alone as an international peacekeeper. Other states, coalitions, or international organizations can step into the breech even if only for purposes of providing a cloak of legitimacy for superpower intervention.

How effectively have international organizations responded to these openings, especially under conditions of ethnic strife? This book attempts to answer this question. It is divided into four parts: The first establishes a theoretical/historical baseline for examining the nexus of international organizations and ethnic strife, the second and third treat two important cases, Lebanon and Yugoslavia, while the last serves as a Conclusion.

It is clear from the opening chapters that there does not exist a theory of international organizations apart from that of international regimes. Put another way, virtually all of the chapters, especially those in the first section, struggle with the incapacity of international organizations to operate effectively, except as vehicles of one or another great-power interest. Thus, realism is a most unwelcome 800-pound gorilla that dominates the theoretical landscape. The best that Milton Esman and Shibley Telhami can do in this regard in their excellent introductory chapter is to allow that there may exist a modest degree of incrementalism in the recent movement of international organizations away from the protean grip of superpower actors. To their credit, they do not overemphasize the emergence of new international norms as the result of international organization actions, such as intervention in ethnic strife, but assert that these norms, however modest in scope, nevertheless contribute a real change that can be built upon in the future. Esman himself provides a useful historical survey of post-World War II interventions by international institutions, followed by Jack Donnelly's wide-ranging attempt to develop a typology of conditions under which international organizations intervene in ethnic strife. Raymond Hopkins closes this section with a perceptive analytic treatment of the necessity for new norms of international organization functioning in response to the recent emergence of ethnic strife. Even the meaning of sovereignty in relation to humanitarian intervention might have to be reconstructed somewhat, albeit, in keeping with the remaining authors, he recognizes the difficulties in such implementation.

The sections on Lebanon and Yugoslavia give relatively complete historical accounts of these tragedies, including the incapacities of the regional and global international institutions to significantly affect the course of these conflicts. In Samir Khalaf's account, we witness in detail the nineteenth-century roots of the Lebanese malaise. Michael Hudson and Naomi Weinberger, in their respective chapters, bring us up to date. Hudson dwells on the domestic context, reasons for the failure of various international efforts, and his important observation that international organization interventions are most likely to succeed either at the beginning of a crisis or at the end, after all parties are exhausted and have clearly seen the limits of what can be attained by force. Weinberger enumerates a highly useful set of variables and conditions for the effectiveness of international intervention.

The treatment of Yugoslavia highlights the pitfalls of trying to analyze an ongoing conflict even as a book is going to press. Nevertheless, there are useful, even prescient, insights to be seen here. V. P. Gagnon gives a vivid and compelling account of the fall of Yugoslavia, presumably as the result of alliances between politico-economic conservatives and nationalists, at times both found in the same person. In Susan Woodward's highly analytic account, we find the failure to intervene effectively by European institutions in the early stages of the conflict precisely in the transition between the old order and a newer one not yet fully formed. In the rapid disappearance of communist East Europe and the reunification of Germany, and the evolution of the EC into the EU, we find an incapacity to act as a consequence of the rapidity of political change. Steven Burg creatively suggests that transitional international authorities might be effective in peacefully managing the deep social and political changes attendant on the emergence of new multiethnic states. This is an example of the proactive approach to such crises that Burg advocates.

It is in Barry Preisler's comparison of the Lebanese and Yugoslav cases that we see most clearly both an analysis overtaken by recent events and the kernel of a correct prediction. Preisler concludes that the Bosnian conflict would be extraordinarily difficult—perhaps even impossible—to resolve peacefully, yet he suggests that only in the intervention of a powerful actor fairly remote from the locus of the conflict is there some hope of peaceful resolution. This is precisely what has happened in the recent intervention by the United States with its NATO allies in tow. The virtues of an explicitly comparative analysis, the only one in the book, are here evident. Telhami then succinctly summarizes the various contributions of the individual chapters and possible agendas for future research.

Overall, this book gives a good account of recent events and the attempts of both international institutions and the scholarly international relations community to come to grips with them. Theoretically, it is thin, not so much as a result of the failure of international relations theorists, but more because of the nature of the material itself. At least for the foreseeable future, international organization activities will continue to be dominated by international forces far more powerful than the organizations themselves. The operation of these forces still requires empirically valid explanations, despite the considerable advances made in recent years.

**Greening Environmental Policy: The Politics of a Sustainable Future.** Edited by Frank Fischer and Michael Black. New York: St. Martin's Press, 1995. 221p. $49.95 cloth, $19.95 paper.

**The Politics of Global Atmospheric Change.** By Ian H. Rowlands. Manchester: Manchester University Press, 1995. 276p. $69.95 cloth, $29.95 paper.

Karen T. Litfin, *University of Washington, Seattle*

These provocative books attest to the fact that the recent revitalization of environmental policy at all levels of political organization has kindled a "greening" of political science. Of the two, *Greening Environmental Policy*, with its twelve contributions from political economy, public policy, international politics, and political theory, is the more wide-ranging and interdisciplinary. Ian Rowlands's comparatively focused and rigorous book, in contrast, will appeal primarily to international relations scholars with an interest in theory-building.

*Greening Environmental Policy* grapples with sustainability beyond the narrow economistic and technical parameters to which it is often confined, thus the term *greening*, which entails participatory politics as much as ecological sustainability. While the book is not always cohesive, most of the essays offer some memorable highlights. In Part I, which explores broadly the discourse of sustainability, Douglas Torgerson eloquently contrasts the 1970s emphasis upon the rejection of endless industrial progress with the contemporary language of sustainable development. Although he does not spell out the implications, his resurrection of John Stuart Mill's formulation of the stationary state as the goal of progress suggests that sustainability can be consistent with liberalism. Using Foucault's notion of governmentality, Timothy Luke very nicely carries Torgerson's analysis forward, arguing that a technocratically oriented sustainability must oversee the normalization of not only people, but also nature. After these two skeptical renderings of sustainable development, the more optimistic reading by Hector Leis and Eduardo Viola of "ecologism," which links democracy, economic equity, and ecological holism, is refreshing, even if not wholly persuasive. While there is some evidence for their claim that the "globalization of ecologism is irreversible" (p. 46) and will provide the basis for a North-South partnership, those who focus upon the trends outlined by Torgerson and Luke will take a darker view.

Part II focuses on industry and technology in environmental policymaking. Michael Black's incisive case study of California's salmon industry explores the ever-increasing costs and the ultimate futility of technological fixes in the search for sustainability. The essay on NAFTA demonstrates that while the treaty's adoption, even with its weak side agreement, can be interpreted as a defeat for environmentalists, the debates surrounding it provide evidence that local environmental justice movements can address globalization. The essay on Chile touches upon the issue of ecology and democracy more obliquely, suggesting that the country's weak environmental policy is substantially due to the industry-friendly institutional mechanisms left in place from years of military dictatorship.

Part III explores environmental values, institutions, and participatory practices. Maarten Hajer's discussion of acid rain in Great Britain suggests that science is mobilized by competing discourses, but that winning the discursive war is not enough; the winning discursive coalition also must have institutional power. Frank Fischer's piece on toxic waste policy and NIMBY addresses the environment-democracy link in a concrete and innovative way, exploring participatory risk assessment as a way of bridging the gap between citizens and technical information.

From a theoretical vantage point, most of the authors take democracy and ecology to be either inherently interconnected or separate but intrinsically desirable ends in themselves. Both of these perspectives run contrary to the claim advanced by some that true sustainability will require a greater centralization of power and perhaps even authoritarianism. A few of the essays demonstrate an anecdotal link between ecology and democracy, but the conceptual underpinnings of that link remain unexplored. Given that the industrialized democracies are arguably the least sustainable societies on the planet, the link between ecology and democracy should not be taken for granted. Yet, only Robert Paehlke's essay directly tackles this question, and then only for a page. Moreover, his claim that the link holds when affluence has been achieved and "where population levels have not yet outstripped ecological underpinnings" (p. 140) is problematic on two counts. First, it says nothing about the prospects for democracy in most of the world. Second, it ignores the shadow ecologies that make possible the industrialized democracies' affluence and permit countries with equivalent population densities, such as Belgium and Bangladesh, to enjoy vastly different lifestyles. Thus, *Greening Environmental Policy* offers many crucial insights but only walks a few steps down the road toward tying ecology to democracy.

Where *Greening* offers an exciting smorgasbord, Ian Rowlands provides a well-structured meal. This highly readable study of international environmental cooperation combines theoretical inquiry with a solid empirical overview of two important regimes: ozone depletion and climate change. The author does an exemplary job of conveying the central information on the cases with ease and humor, and without the verbal tedium that so often accompanies analyses of technical negotiations.

The theoretical framework outlines three broad sets of explanations for international cooperation—power-based, interest-based, and knowledge-based—each of which has some explanatory value. From these, Rowlands derives four hypotheses to be tested in the body of the book. First, a scientific consensus is prerequisite to international cooperation. Second, the key actors must calculate the benefits of cooperation to be greater than the costs. Third, North-South equity issues must be adequately resolved as a condition for international cooperation. Fourth, prospects for cooperation are enhanced by the presence of any one of four catalysts: political entrepreneurs, nongovernmental organizations, issue tangibility, and a conducive international environment.

The evidence neither fully confirms nor fully contradicts the hypotheses, leading the author to accept them as mutually intertwined heuristic devices. For instance, the relationship between science and politics is not unidirectional; science turns out to be at least partly constituted by interests. Likewise, Rowlands reflects upon whether equity issues are really shorthand for questions about interests. In the end, he offers an insightful self-critique in which he admits that he should have paid more attention to the mutual interaction among the hypotheses.

Given the prominence of power-based explanations in the general literature, it is surprising that none of the hypotheses qualifies as such. A good realist, one might imagine, would argue that U.S. leadership was the primary impetus for the Montreal Protocol on the ozone layer and that U.S. resistance to a strong climate change convention effectively precluded cooperation. While Rowlands claims that his hypothesis about equity relies upon one variant of power-based explanations, dependence theory, he does not spell out the relationship.

A deeper analysis would show that science and equity issues can be blurred, as they were in the climate change discussions. Rowlands takes greenhouse gas emissions data for granted, but there has been a lively debate over how to measure responsibility for emissions—a debate that is fundamentally about equity. Indeed, there is always a relationship between science and equity whenever developing countries lack the resources to be fully informed. One understated implication of Rowlands's work is that ethical considerations can contribute to international cooperation, a fact not sufficiently acknowledged by either realists or institutionalists. The debates, for instance, on the technology transfer funds for both ozone and climate change were simultaneously scientific and ethical debates.

The book contains a few oversights. The author makes a point of arguing that the Antarctic ozone hole was crucial in bringing about international cooperation, but he never addresses the fact that the Montreal Protocol negotiators explicitly agreed to ignore the hole because its causes were not adequately understood. The question, then, is how an unacknowledged factor nonetheless paved the way to cooperation.

In contrasting ozone science with climate change science, the author wrongly claims that the former was undertaken independent of governments, while the latter was not. Yet, UNEP's Coordinating Committee on the Ozone Layer did no independent research, but was rather a channel for disseminating information abroad. Indeed, the United States accounted for at least 80 percent of the world's ozone science, which realists might claim made it something of a scientific hegemon. If government investment in science is substantially influenced by interests, then a three-way link among power, interests, and knowledge might be explored.

On the issue of catalysts, Rowlands finds that NGOs were not particularly influential on either issue, but this could be due more to his ways of measuring their involvement than their actual unimportance. While he is concerned largely with their direct participation in negotiations, their less tangible role may have been in helping to create "a conducive political environment." He admits that "states did not initiate demands for cooperation" (p. 259), but he pays little attention to nonstate actors who did initiate such demands. Indeed, the sort of discourse coalition that Hajer finds with respect to acid rain in Britain might also be uncovered for ozone and, to a lesser extent, climate change.

Lurking behind the scenes throughout the book is the question of the state, which emerges only at the end. The author suggests that the state is "the key dependent variable" (p. 258), yet a major message of the analysis is that there were neither independent nor dependent variables. Everything seems to vary with everything else, yet the author seems reluctant to accept the radical consequences of his own study. He cites the need for a multidisciplinary approach (p. 265), but himself relies on a state-centric analysis based upon traditional theoretical categories. Yet, by taking the conventional modes of exploration to their limit in a thoroughgoing manner, Rowlands has provided an invaluable service in laying the groundwork for future research efforts.

**The New Untouchables: Immigration and the New World Worker.** By Nigel Harris. New York: I. B. Tauris, 1995. 254p. $35.00.

Anthony M. Messina, *Tufts University*

*The New Untouchables: Immigration and the New World Worker* by Nigel Harris is the latest in a spate of new books that speak to the origins, effects, and likely future trajectory of international labor migration. Like many of these recent efforts, the focus of *The New Untouchables* is global. It is global in two senses. First, it locates the phenomenon of mass immigration within the context of a rapidly liberalizing and integrating international economy. It is within this context that Harris argues that there is currently a severe contradiction "between segregated national spaces and the increasing international movement of workers driven by the world labor market" (p. 2). Second, *The New Untouchables* is also global in that its point of reference and its evidence are not country-specific. Rather, in developing his central arguments Harris impressively draws upon numerous country cases and selected empirical evidence from throughout the developed and developing world.

Simply stated, the central argument of Harris's book is that the circulation of an extraordinary volume of workers across states and traditional regional boundaries is the direct consequence of the ever-accelerating integration of the world economy and the partial emergence of a global labor market. From this thesis several subarguments flow. First, all the advanced industrialized countries require immigration to sustain economic growth. According to Harris, the historical record suggests that even modest rates of economic growth inevitably lead to a demand for labor that will sooner or later exceed domestic supply. Second, the mass migration of labor collides with the prerogatives of traditional state political sovereignty. In this inevitable collision, the state must decide whether to defend its traditional prerogatives or sacrifice domestic economic growth and the health and expansion of the international economy. Finally, whatever their political inclination or complexion, national governments have but limited authority to interfere with the domestic demand for foreign labor. A number of factors in the domestic economy conspire to keep the demand for immigrant workers relatively robust even under conditions of high "native" unemployment.

As virtually any serious student of Harris's subject will immediately recognize, neither the central nor the subarguments of *The New Untouchables* are particularly novel. Although Harris may give them a somewhat different twist, each thesis essentially reformulates an argument that has been previously advanced sometime during the past fifteen years or so by literally dozens of scholars working within the subfield of immigration studies. What is original, however, is Harris's attempt to synthesize and refine these arguments in order to propagate the view that mass migration is universally beneficial. While conceding that it generates costs, Harris ultimately concludes that mass immigration economically benefits all parties: for the immigrant-receiving countries the payoffs are greater and sustainable economic growth and an adequate and flexible labor supply; for the immigrant-sending countries the benefits are a return stream of remittances and a more productive work force; and for the immigrants themselves the advantages are greater material prosperity and liberation from the social prejudices and constraints of their home societies.

How convincing is this perspective? Unfortunately for Harris, the issue of whether or not immigration is of net benefit to the immigrant-sending or immigrant-receiving countries or even to the immigrants themselves can never be definitively settled. It can never be satisfactorily resolved, because there is no research methodology that can ever capture, let alone quantify, all the pertinent costs associated with mass immigration. Consider, for example, the obvious social and political costs of immigration, many of which are transparent even to the ordinary citizen. These costs very often include greater social and ethnic conflict, the expansion of xenophobic and right-wing populist movements, and political pressures on the domestic social welfare consensus. Although Harris is cognizant of these costs, he more or less (wrongly) dismisses them as petty. For Harris, a liberal international migration regime is simply justified economically and, thus, domestic anti-immigrant popular sentiment, ethnic conflict, and mass xenophobia are but inconvenient obstacles in the path of a rational domestic economic policy and a set of optimal economic outcomes.

This criticism does not overshadow the strengths of *The New Untouchables.* Indeed, there is much to recommend in the book. In addition to its global approach, the book is very well informed, reasonably well written and argued, comprehensive in its coverage of issues pertinent to the multifaceted phenomenon of mass immigration during the late twentieth century, and quite often insightful. It unquestionably makes an important and timely contribution to current policy debates concerning the role of mass labor migration in fueling domestic and international economic growth. However, in the end, the book is perhaps too global and ambitious for its own good. Although Harris's perspective on international labor migration as universally beneficial is probably correct, this view dismisses its established and potential critics too easily. By inadequately considering the wider range of noneconomic costs associated with mass immigration and by demonstrating insufficient respect for those scholars and policymakers who do, *The New Untouchables* ultimately fails to break new intellectual ground on its increasingly important subject of study.

**The Eritrean Struggle for Independence: Domination, Resistance, Nationalism, 1941–1993.** By Ruth Iyob. Cambridge: Cambridge University Press, 1995. 198p. $49.95.

Edmond J. Keller, *University of California, Los Angeles*

The aims of this book are threefold: (1) to understand why the former Italian colony of Eritrea was denied its right to self-determination at a time when most European colonies in Africa were securing their independence; (2) to understand the strategy employed by imperial Ethiopia to have its claims to Eritrea recognized and upheld in the international community, and how Eritrean nationalists responded to the hegemonic project of the imperial state; and (3) to disentangle the process by which the Eritrean movement for national liberation was formed, how it developed, and how it ultimately emerged victorious against Ethiopia.

The book is divided into two parts. The first, borrowing from the international relations literature on regional hegemony, sketches the author's framework for analysis. In the second part of the book Iyob concentrates on the dynamic of the organizational construction of the main Eritrean nationalist movements between the 1940s and 1980s, cogently discussing why the various movements that took up armed struggle were able to form a united front that successfully threw off the yoke of Ethiopian domination.

Part I begins with a brief review of the relevant literature on Ethiopia-Eritrea relations. Iyob's intention is not to write yet another work that seeks to correct previous historical inaccuracies; instead, she is more concerned with situating her own work within the context of the extant literature. She divides that literature into two main schools. The first subscribes to a "Greater Ethiopia" thesis, which holds that Eritrea is a part of historic Ethiopia and therefore does not have an inalienable right to self-determination. The second school looks upon the Eritrean case as unique in international law. It views the annexation of Eritrea by Ethiopia in 1962 as a violation of the right of the Eritrean peoples to self-determination. Although the author endorses the latter perspective, she argues that scholarly attention would be more fruitfully focused on the dialectic of hegemonic domination and resistance as is represented in the Eritrean war of national liberation. This is the primary focus of the book.

In constructing her analytical framework, Iyob is informed by three bodies of literature dealing with the concept of hegemony: the realist school of international relations, world systems theory, and the cultural hegemony school as represented in the work of Antonio Gramsci and Michel Foucault. Her thesis is that imperial Ethiopia pursued a hegemonic project that involved establishing itself as a leader in the Pan African movement, thus deflecting attention from the fact that by annexing Eritrea into the modern Ethiopian state it was engaging in "late colonialism." In order to do this, the imperial state had to engage in skillful diplomacy not only in the region but also in the international community as well. Ethiopia thus became, from the very founding of the Organization of African Unity (OAU), a leader in that body, and it entered into strategic alliances with the United States and other Western powers at the UN to win support for its position on the Eritrean question. In the process, Ethiopia benefited greatly from regional and international consensus among states that existing territorial boundaries in Africa were sacrosanct. Ironically, Ethiopia annexed Eritrea only five months prior to the founding of the OAU.

Iyob argues that Ethiopia's ability to establish and maintain its diplomatic capability and legitimacy in large measure was made possible by its successful construction of a "hegemonic regime of truth." This involved Ethiopia's leaders inventing a logic that justified a denial of Eritrean claims to self-determination while articulating a logic for Ethiopian claims. The fabrication of this situation is described through an analysis of primary documents from the UN, OAU, Ethiopia, and Eritrea and through personal interviews with key players during the nationalist and immediate postnationalist period. Iyob strongly criticizes the rest of Africa for having bought into Ethiopia's "regime of truth" virtually without question, suggesting that this widespread acceptance was why the Eritrean struggle was so protracted.

The second part of the book concisely details the rationale for the armed struggle from the Eritrean perspective. During the nationalist period, the nationalists were virtually evenly split along religious and secular lines. After a brief period in which the secular nationalists of the Eritrean Liberation Movement (ELM) tried to take the lead in pressing for Eritrean independence, they were repressed by the imperial government and quickly replaced by the Muslim-led Eritrean Liberation Front (ELF). Some Christians sought to work within the ELF; but because of the movement's close ties to conservative Arab states, it splintered. Furthermore, tensions within the organization emanated from the fact that the ELF was made up of a collection of patron parties whose leaders mistrusted each other. The most significant splinter organization to emerge was the Eritrean People's Liberation Front (EPLF). This organization was able to pick up the mantle of the secular-based ELM, and by the early 1980s, after years of internecine warfare with the ELF, it reigned supreme among the Eritrean movements. It was able to construct an international network of Eritrean supporters and to build an effective fighting force on the ground. As the Cold War melted down, Ethiopia and its troops became demoralized by the length of the war and some significant military setbacks. In late May 1991 the EPLF was able to vanquish the Ethiopians on the battlefield.

Significantly, the growth in the military strength of the EPLF coincided with a new diplomatic initiative in the late 1980s that forced the international community to rethink Eritrea's claims to self-determination. As this happened, Ethiopia's "regime of truth" evaporated. A major point made in this discussion is that rather than having been always in existence, a sense of Eritrean nationhood was constructed among that country's nine distinct ethnic groups only during the course of struggle.

This book is an excellent contribution to the extant literature on Africa's newest independent state. It is remarkable in that, although it is written by an Eritrean national, it moves beyond the generally partisan and polemical pieces that tended to characterize discussions of the

Eritrean question after World War II. At the same time, the work could have been stronger if it had more discussion of the strategy employed by the imperial state in developing its hegemonic project. In addition, rather than concluding with a speculative chapter on "Building the Eritrean Polity," the author would have been better served by a return to the primary thesis of the book. These shortcomings notwithstanding, this work is must reading for students of the politics of the Horn of Africa.

**Russians in the Former Soviet Republics.** By Paul Kolstoe, with a contribution by Andrei Edemsky. Bloomington: Indiana University Press, 1995. 340p. $37.50.

Rasma Karklins, *University of Illinois at Chicago*

The collapse of the Soviet Union left twenty-five million Russians living outside of Russia. In this sweeping survey, Kolstoe compares the differential adaptation of this new Russian diaspora in the fourteen non-Russian successor states of the USSR. It is the advantage of his comparative approach that it allows us to discern what changes are due to the imperatives of political and economic transition, and what is due to the policies of one or the other of the governments. Even though Kolstoe debates the policies of the various states, he tends to conclude that many processes in the diaspora are related less to politics than to socioeconomics. A prime example is the return of Russians from various regions to Russia. Although Turkmenistan is said to have the "best" policy toward local Russians (p. 218), many more Russians leave from there than from Estonia.

Appropriately, the study often discusses migration, both as it relates to the origin of the diaspora as well as to its gradual contraction since 1990. Part I presents a survey of migratory patterns of Russians since 1500, including the state-sponsored demographic colonization of Russia's neighbors. The motives for such resettlement included security considerations; but ironically the opposite end was achieved, since the borderlands were a frequent source of rebellions spreading to the Russian heartland (p. 25). Ultimately, imperial overextension contributed to the fall of the Soviet system.

The Soviet demise brought a reversal of the out-migration of Russians to non-Russian territories. As is true for Kolstoe's discussion of the motives for the original waves of Russian migration, competing explanations are discussed. He notes that while earlier non-Russian authors mostly pointed to state-orchestrated settlement based on ethnopolitical strategies, such arguments are now used primarily by Russians. Ethnopolitical arguments tend to focus on the Russians having experienced a loss of their former privileges, including language hegemony. In contrast, non-Russians today more often argue that migration is motivated by personal or economic considerations.

In addition to two chapters surveying the growth of the Russian diaspora between 1500 and 1989, Part I includes a chapter discussing its changing status in non-Russian union republics during the Soviet era. Kolstoe presents an adept summary of existing studies but little new insight. Part II consists of five chapters reviewing policies in individual states. Part III contains an outstanding chapter on Russia's policy toward the diaspora (this chapter is written with Andrei Edemsky) and a concluding chapter on policy options and future scenarios.

The chapters differ in quality, in part due to the differing depth of original research. Kolstoe himself laments the dearth of original material available to him in regard to Soviet Asia, but gaps are evident elsewhere as well. Analytically, his best case study is that of Kazakhstan. Kolstoe manages to show how the Kazakhstani Russians have forged a clearly defined role in the post-Soviet politics of this new state.

The best chapter by far—and also the most topical one—deals with Russia's changing policies toward the new diaspora. The incisive analysis highlights changing policies of various Russian leaders, many of whom are concerned less about the fate of their co-ethnics than with exploiting a sensitive issue. Parallels are drawn to the way that Hitler exploited the German populations in Eastern Europe. We learn about the calls for use of force and economic blackmail made by the notorious Zhirinovsky, but less well known examples include the Russian military establishment's exploitation of alleged discrimination against Russians abroad to lobby for higher budgets.

In regard to policy options, Kolstoe suggests that Russian diaspora members choose to become integrated minorities who—while retaining their cultural identity—are staunchly loyal to their new states. He notes that many Russians have yet to accept this solution, but he always takes care to distinguish among numerous subgroups in the diaspora, a group that cannot be referred to as a single bloc of people.

Appropriately, Kolstoe uses the concept of the Russian diaspora rather than a national minority, yet he is inconsistent in his awareness of the consequences of the assumption of one or the other status. As he notes (p. 308), international conventions focus on the protection of the rights of indigenous historical minorities rather than immigrant or settler groups; yet at times Kolstoe writes as if no distinctions apply. However, he is clear in showing that many members of the Russian diaspora are confused about their identity. While some have identified with their new states and are willing to see themselves as Kazakhstanis, for example, many others identify with Russia or the defunct Soviet Union. Difficult as the task may be, the new states need to build a community of citizens with a clear sense of belonging.

Sometimes Kolstoe's sources are limited. He himself laments the scarcity of original material available to him on Soviet Asia. In his chapter on Estonia and Latvia, he claims to have surveyed "the press and public discourse" (p. 107), yet references are skimpy. As a result, he presents the policies of Estonia and Latvia too harshly—for example, when he wrongly states that they have not provided state-run language courses or that employees failing to pass language tests in Latvia "were to be fired" (p. 127). The language law of Latvia makes no such stipulation. All it says is that employees may be let go if poor language facility impedes the performance of professional duties—for example, if a policeman or doctor is unable to respond to a call for help in Latvian. In practice, such layoffs have been rare.

Kolstoe deserves credit for tackling a complex and highly topical issue. Nevertheless, the study illustrates the dangers of writing on contemporary topics, especially when societies undergo profound transitions that rarely proceed in a linear fashion. Many of the developments described have been superseded, most notably so in the case of Belorus, which has been moving back into the Russian fold. Other statements were outdated soon after the author wrote them in 1993, for example, that Latvia has no citizenship law (rather liberal provisions for naturalization were passed in August 1994). Although a second edition of this book may be unlikely, it would be marvelous to see an updated and revised version in a few years.

**Closure in International Politics: The Impact of Strategy, Blocs, and Empires.** By John A. Kroll. Boulder, CO: Westview, 1995. 289p. $59.95.

Carolyn Rhodes, *Utah State University*

This book offers a somewhat unique approach to the explanation of stability in international trading systems. Rather than focusing on the characteristics of the system as causal factors explaining periods of relative trade openness and cooperation and periods of relative trade closure and conflict, it identifies national policies as the crucial factors that contribute to the ebb and flow of international trade cooperation. Arguing that (large) trading nations find themselves facing the incentives of a prisoner's dilemma game, in which the temptation to raise optimal tariffs against other trading nations constantly threatens the stability of an open trading system, the author attempts to identify those circumstances under which these nations will be more likely to resist that temptation. He then applies this framework to the historical trading relationships of Britain and Germany during the nineteenth century. Based on the assumption that optimal tariffs are more likely when nations form empires or customs unions, the book's ostensible purpose is to explain the deleterious effects that empires and customs unions have on the system of trade openness.

Despite a useful review of the existing debate within international political economy regarding the conditions necessary and sufficient to achieve and maintain trade openness, and a rigorous effort to explain how the ability to impose optimum tariffs constitutes a prisoner's dilemma in international trade relations, the book falls short of its ambitious goals. There are a number of problems that the author creates for himself, and they are unfortunate, because many of the insights offered are useful, but their effect is reduced by the overall inadequacy of the framework employed. This is particularly disappointing to me, because I agree with Kroll's basic premise that state policies do matter and that they do affect the stability of the free trade system. Unfortunately, the book has so many inadequacies that the message is undermined. At the heart of these shortcomings is his depiction of international trade as a prisoner's dilemma, where the temptation to impose optimum tariffs to enhance national income at the expense of trading partners characterizes trade relations. Fundamental to the persuasiveness of this depiction would be an analysis of actual trade interactions that demonstrates the widespread ability of importing nations to impose optimum tariffs, yet the author does not include such an analysis. Because optimum tariffs require monopsonistic purchasing power on the part of the importing nation, they are not common features of trade policy. It is unlikely that the temptation to utilize them has been widespread over time. I suspect, in fact, that as a form of protectionism optimum tariffs are probably quite rare, and the author offers no information to convince the reader otherwise.

Similarly problematic is Kroll's failure to convincingly connect his theoretical discussion regarding prisoner's dilemmas and optimum tariffs to the historical material that he claims supports it. This is crucial, because the foundation of the entire book is the assumption that trading nations use optimum tariffs whenever possible to improve national income at the expense of their trading partners and that this creates a prisoner's dilemma situation in trade. This notion may be plausible in theory, but nowhere in the book does Kroll demonstrate that national policies are produced from this incentive, and nowhere in his historical accounts of Britain and Germany does he examine actual uses of optimum tariffs. While he assumes that larger states—defined under his terms as states that "possess the power to shape markets" (p. 29)—are the ones caught in this prisoner's dilemma, he notes that "small states, even though they are small in the world-wide market, may be able to impose optimum tariffs if they are able to find target states that are highly dependent upon them" (p. 64). Yet, in his empirical investigation of the actual operation of trade relations during the nineteenth century he fails to demonstrate which states and under what circumstances optimum tariffs were actually utilized.

Kroll further asserts that colonial expansion increases market control on the part of the metropole, thus creating greater power to exploit optimum tariffs; yet he never demonstrates that optimum tariffs were ever utilized. In fact, in his review of British commercial policy during the nineteenth century, he admits that preferential tariffs were not characteristic of British colonial expansion (p. 157). His historical chapters are particularly weak in establishing the dynamic he claims characterizes the prisoner's dilemma (and its avoidance during the zenith of free trade). He claims that other powers moved toward free trade in the early to middle nineteenth century and that Britain "matched" those moves, implying a reciprocal dynamic initiated by others. Yet, his own historical account notes that Britain's initial moves toward free trade were unilateral shifts due to major ideological changes regarding the value of cheaper imports for maximizing British income. Throughout this book, Kroll claims that his perspective is generally one guided by classical economics; however, his persistent reliance on the argument that nations constantly seek to maximize income through optimum tariffs reveals a mercantilist perspective that downplays, if not ignores, the value of free trade (even unilateral at times) for the purpose of maximizing national income.

When the policy action does not fit his theory, he either downplays it or attempts to characterize it as unconventional. For example, later Britain consciously moved to control a larger empire in order to maintain a free trade system beyond the grasp of the increasingly protectionist continental powers—a decidedly cooperative move in strict prisoner's dilemma (PD) terms as he described it earlier (i.e., no trade closure action). Kroll maintains that this was actually a move toward trade closure. In an effort to make history match his theory and lacking the evidence to make this claim, Kroll argues that Britain was "perceived" to be moving toward imperial preference (defection, in PD terms), and this prompted its trading partners to retaliate (p. 158). Citing speeches by Bismarck calling for Germany to retaliate as evidence of this interpretation, he fails to produce actual evidence of British defection. Moreover, he ignores the political climate of the time, a climate characterized by German ascension as a powerful nation-state, determined to rival the influence of the British empire regardless of British trade policy.

Even though the theoretical discussion rests on assumptions regarding large states and optimum tariffs, Kroll fails to demonstrate under what conditions optimum tariffs have actually been used, and he totally ignores the possibilities and effects of other forms of protection. If states pursue protection only to maximize income, why has protection been so widespread under circumstances that do not produce this effect? An entire dimension of protectionism is missed. At a minimum one needs to differentiate between optimum tariffs and other tariffs aimed at protecting certain politically powerful domestic interests, yet Kroll has made no attempt to do so here. Moreover, his explanation of the strategic policies of trading states ignores domestic sources and domestic effects of those policies, as well as effects on foreign domestic interests. While this is an intentional omission aimed at focusing the reader's attention on the character of the policies rather than where they came from, the problem arises because there is not a clear picture of how trade power is exercised. His assumption is that trade power is always exercised strategically by the state to enhance its interests as a whole, when in reality certain domestic interests may not be considering national income effects (which is how he defines state interests) when they demand protection or other trade action. He criticized others who "too often . . . assume that the protection of domestic producers, rather than the maximization of national income, is the sole motivation for retaliation" (p. 95), yet he commits the opposite error. His discussions totally ignore the actual character of trade, what commodities were traded, where liberalization occurred and didn't occur, and what the specific patterns of trade were. This significantly reduces the persuasiveness of his argument, because it is difficult to ascertain whether the cause-effect relationships he claims are actually in evidence.

Kroll repeatedly risks tautology in his discussion of independent and dependent variables. Basically, he argues that protectionist policies produce protectionism, a hardly informative observation. This is most graphically illustrated in a table (p. 23) where he notes that "free trade is stable when states lower their tariffs" and "economic closure increases when states raise their tariffs." Although his discussion in chapter 3 of the policies that appear necessary and sufficient to maintain a stable free trade system are much more lucid and interesting (system binding, contingent discrimination, and avoidance of end-games), the historical discussion again demonstrates his difficulty in distinguishing between specific policies as causal factors and policies as outcomes. Moreover, by refusing to systematically examine the motives behind the policies (what produced them) and to differentiate among them in a clear assessment of trading states' interaction, the author denies us a clear understanding of why and how these three factors contribute to stability.

Kroll also fails to consider the role in trade relations of investment, capital, and the character of the commodities traded. Total market demand and its potential effect on world market prices are the only factors identified as endowing power in a given trading relationship. This is a very limited view, since no one can deny the power that Britain had in the world economy due to both its financial surplus and the kinds of products it purchased and sold. In today's terms this bias in favor of purchasing power is especially peculiar since it would deny that Japan has considerable national income that it strategically pursues despite its limited ability to impose optimum tariffs. Additionally, his assumption that colonial expansion created greater market power for Britain (in terms of its ability to apply optimum tariffs) is undermined by the fact that colonies tended to purchase different products than did Britain.

Finally, his argument at the outset that the current world trade system is at risk due to customs unions that are analogous to the empires of the latter nineteenth century is not at all the focus of the book, and although his Conclusion returns to this assertion, he never establishes the necessary parallels to be convincing. In fact, in the concluding chapter he even notes why GATT Article 24-style customs unions have not been the same as earlier preferential arrangements under empires, yet he persists in his undocumented assertions (which fly in the face of contemporary trade data and free trade initiatives) that customs unions imperil the current free trade system. Consequently, this analogy appears piggybacked onto the original project without being effectively integrated into the theoretical and comparative analysis. Without systematic comparisons with the theoretical and historical material presented, its value is meaningless.

**The Third World in Global Environmental Politics.** By Marian A. L. Miller. Boulder, CO: Lynne Rienner, 1995. 181p. $17.95 paper.

Amitrajeet A. Batabyal, *Utah State University*

Near the end of World War II, President Roosevelt remarked that in future days he looked forward to a world founded on four essential freedoms, including "freedom from want." While "freedom from want" has remained the dominant development theme in the post-war era, the perception of what constitutes "want" has shifted over time. Increasingly, researchers (e.g., Batabyal, *Ecological Economics*, 13 [1995]) have agreed that not only is there a significant environmental component to want, but also this component essentially links the welfare of less-developed countries (LDCs) and developed countries. As a result, it is how understood that policies attempting to ameliorate the problems created by want in LDCs must also address the linkage issue arising from the environmental dimension of want. It is essentially this recognition that has generated a significant amount of research effort (e.g., Haas, Keohane, and Levy, *Institutions for the Earth* [1993]) directed toward comprehending how international institutions might enable us to use our fragile environmental resources appropriately.

While a considerable amount has been written (e.g., Bernauer, *International Organization*, 49 [1995]) about the design and functioning of international environmental institutions, most of this writing has paid scant attention to the role and significance of LDCs in this undertaking. Marian Miller's book fills this lacuna in the literature by focusing explicitly on the role of LDCs in global environmental politics. In this book, she makes two key points. First, because of the economic *and* ecological interdependence that typifies most interactions between developed countries and LDCs, the latter group of nations have had and will continue to have some bargaining power in determining what international institutions can and cannot do when faced with a specific environmental problem. Second, because of the asymmetric distribution of power in the world, unless LDCs are constantly vigilant, their rights will systematically be usurped and their interests paid no attention to by the "cabal" of developed country governments and multinational corporations (MNCs).

In support of these two points, Miller uses a modified realist approach that she calls the "regime approach" to document LDC–developed country interactions that have led to international environmental agreements (IEAs) dealing with ozone protection, trade in hazardous wastes, and biodiversity. In discussing each of these IEAs, Miller begins by providing some history of the underlying issue. She then discusses the roles of the various actors and ends by providing an account of the extent and nature of participation by LDCs. Miller correctly observes that in two of the three IEAs under study (ozone and biodiversity), were it not for the salience of *ecological* interdependence, LDCs would have had little or no bargaining power in determining the outcome of the IEA. This insight notwithstanding, this part of Miller's study is somewhat long on facts and short on analysis. For instance, there is no systematic discussion of the role that international institutions can play in fostering an hospitable environment for the creation and maintenance of IEAs. Almost completely neglected is the role that international institutions can play in building the political and administrative infrastructure of LDC governments. Instead, what we do get is a starkly adversarial depiction of LDC interests, on the one hand, and developed country and MNC interests, on the other. One comes away with the distinct impression that Miller views the IEA-formation process as a zero-sum game.

On a related note, Miller observes that in many instances the success or failure of an IEA is dependent on the participation of select LDCs like China and India. This point is significant and should have been developed at greater length. Furthermore, a systematic discussion of the *internal* politics of these LDCs and the way in which the internal politics has or has not shaped the nation's understanding of environmental problems would have been very helpful. While international institutions can go some distance in helping to create an understanding of the importance of a specific environmental issue, in order to understand the formation of successful IEAs we need to comprehend the interface of domestic politics and a nation's understanding of environmental problems. In this respect, Miller's analysis falls short. Although she does provide the reader with scattered bits of information about China and India, she does not conduct a thorough analysis of the domestic politics of LDCs whose global participation she deems important.

Finally, Miller's conspiratorial view of international agencies, MNCs, and developed-country governments and her unabashedly adversarial perspective on world politics will, if anything, detract from the cause of LDCs. This is particularly true given that many of the strategies she advocates for the promotion of sustainable development in LDCs require the cooperation of precisely those actors that she vilifies in her book.

In conclusion, while this is an interesting book on a very topical subject, I suspect that its impact will be less than considerable. I applaud Miller for conducting a study of global environmental politics from the perspective of LDCs and for raising some interesting and relevant issues, yet I come away more with a feeling of what might have been rather than what is. Whatever the oracular potential of Miller's approach, it most certainly is Delphic in its reticence.

**The Clash with Distant Cultures: Values, Interests and Force in American Foreign Policy.** By Richard J. Payne. Albany: State University of New York Press, 1995. 285p. $59.50 cloth, $19.95 paper.

David Campbell, *Keele University (UK)*

In the many attempts to provide an analytical touchstone to makes sense of and chart a foreign policy course in the post–Cold War period, the concept of culture has emerged as a leading candidate. From the numerous popular observations about its seeming importance to so-called ethnic and nationalist conflicts to the grand claims of the Samuel Huntingtons and William Linds about it being the dividing line of a new international order, the concept of culture has achieved a new place of prominence in foreign policy analysis. From this jumping-off point, Richard Payne has embarked upon a study of the role of American cultural values in shaping United States foreign policy and the likelihood of culture contributing to conflict resolution.

Payne's argument seeks to illuminate and confirm three propositions: first, the United States is less likely to be in dispute with or use force against a country that has strong cultural similarities; second, the greater the cultural distance between the United States and others, the more likely a conflict and the more easily force will be deployed to serve national interests; and third, where "significant interests" are not at stake and the cultural distance is great, the United States is less likely to rely on force or invest resources in negotiations to resolve the conflict. Each of these propositions is explored in a chapter-long case. These cases are the Palestinian-Israeli dispute, the Gulf War of 1990–91, and the Bosnian conflict, respectively.

The trouble with culture, as Payne readily acknowledges, is that it defies easy understanding. Drawing on some well-known sources in anthropology and sociology, Payne settles on an understanding of culture as "a set of shared learned values, beliefs, perceptions, attitudes, modes of living, customs, and symbols" (p. 9). Perhaps more inclusive than it is illuminating, this understanding of culture nonetheless challenges a broadly defined realist perspective through the invocation of factors that are not exclusively international. As such, Payne's study bears affinities to the small but growing literature that subjects conventional foreign policy analysis to critical theoretical investigation. However, its critical imperative is substantially contained by an adherence to conventional social scientific norms that draw some common distinctions (between material and ideational elements, objective and perceived reality) that should be fundamentally challenged by a concern with culture, yet are required by the reliance on a logic of explanation. In this context, Payne's argument resembles Goldstein and Keohane's (1994) contribution to their *Ideas and Foreign Policy* collection, where the unwillingness to accord legitimacy to approaches that depart from the rationalist agenda means the full import of a concern with discourse is diminished. The point is not that dealing with culture or ideas is untimely (though we might want to ask how it was we ever came to think of them as either unimportant or unrelated to foreign policy in the first place). Rather, it is that a thoroughgoing analysis of culture or ideas is more than likely going to expose the limits of a rationalist approach rather than be satisfied by it. To this end, Payne's implicit articulation of culture as a variable that can explicitly and definitively explain certain foreign policy (non)decisions is theoretically underdeveloped.

Nonetheless, in his review of culture's place in the U.S. foreign policy tradition and outlook, Payne has written a useful reminder of the historical dispositions that continue to inform even the most self-consciously realistic policies. By drawing attention to the pervasive but often overlooked importance of the myth of the frontier, the notion of the "City on a Hill," and other messianic proclivities of an avowedly religious nation (constitutional provisions notwithstanding), Payne has effectively set out the parameters within which U.S. use of force can be understood. At the same time, some of Payne's discussion is lacking the necessary historical depth to appreciate the complexity sometimes involved. For example, in the important case of how the Spanish confronted others in their colonies—arguably the foundational moment in international intercultural relations between Europeans and others—Payne wishes to emphasize that, as exemplified by the arguments of Bartolome de las Casas, the Spanish were confident enough of their superiority that they had no need to define themselves in violent opposition to the Amerindians and could thus concern themselves with conversion rather than extermination. As Todorov's (1984) seminal account, *The Conquest of America*—sadly overlooked by Payne—makes clear, the views of de las Casas were widely debated and disputed in both colonial theory and practice. Subsequent U.S. dispositions are thus not quite as exceptional as Payne maintains.

The cases Payne uses clearly demonstrate the importance of culture to a full appreciation of the foreign policy decisions taken. In particular, the arguments that enabled and supported U.S. action against Iraq in 1990–91 vividly demonstrate that any account that overlooks these issues is incomplete. Nonetheless, some questions have to be asked about the cases, especially the chapter on Bosnia. Some of the problems evident here are a function of the way in which events in the region moved quickly after the fall of Srebrenica in July 1995 and Croatia's military operation in the Krajina during August and September 1995. The developments that were set in train, such as substantial NATO air-strikes and the newly found diplomatic determination of the United States, could not have been predicted by a book published in that year, though the argument could have been more open to the possibility of changes. Yet, those events have contradicted the point that case was designed to make, namely, that the cultural distance between Bosnian Muslims and the United States can explain U.S. willingness to take a back seat to the European Union.

More important, the notion that there is cultural distance between the United States and Bosnia depends first of all on the general idea of "cultural distance" and, second, upon the specific notion that the category "Bosnian Muslim" is meaningful. Both are problematic. "Cultural distance" implies that cultures are distinct, stable entities, pre-given prior to contact. No allowance is made for the reality of heterogeneity and hybridity. This limitation is then manifested in a particular way with the idea of "Bosnian Muslim." The conspicuous lack of reflection on the use of this term means that Payne, like most commentators on Bosnia, uses *Bosnian Muslim* without hesitation to signify the majority population of Bosnia. This means, somewhat oddly for a book that is drawing our attention to cultural categories, that Payne's own argument participates in and perpetuates in its own small way the very cultural distance he is analyzing. Yet, much like the tensions in Israeli society between secular and religious understandings of the state, one of the most important political cleavages in Bosnia—before, during, and especially after the recent fighting—is whether the polity and its rulers should be predominantly multicultural or Islamic. Nor is the latter concept fixed. There is considerable debate and tension about what form of Islam, if any, might be the most appropriate for a modern European state. All this is missed by the reductionist and homogenizing notion of "Bosnian Muslim."

Culture is and will remain an essential concept for international relations. As timely as Payne's analysis is in drawing the significance of culture for foreign policy to our attention, a fuller appreciation of the concept will require modes of interpretation better attuned to the historical complexities and political production of cultural categories.

**Democracy and International Conflict: An Evaluation of the Democratic Peace Proposition.** By James Lee Ray. Columbia: University of South Carolina Press, 1995. 252p. $39.50.

William J. Dixon, *University of Arizona*

The "democratic peace" of the title refers to the now well-known observation that democratically governed nation states have never—or at least hardly ever—waged war against one another. This historical pattern of peaceful relations among democracies is not itself at issue, having been confirmed by literally dozens of empirical studies employing different operational criteria over varying periods of observation. What is at issue is the democratic peace *proposition*, the claim that democracies do not wage war against one another precisely *because* they are democracies. James Lee Ray's new book does indeed deliver the evaluation promised by its subtitle and in doing so provides us with a landmark statement on the democratic peace that will demand close attention from the proposition's proponents and opponents alike.

Ray begins in customary fashion with a chapter reviewing the current state of theory and research on the democratic peace. Although existing arguments and evidence are by no means conclusive, they are nonetheless judged sufficiently supportive of the democratic peace to justify continued investigation of its central proposition. This investigation is implemented along three separate tracks in succeeding chapters. In chapter 2 Ray considers the possible global implications of the democratic peace proposition through an examination of historical trends in regime transitions toward democracy. Chapter 3 then undertakes an exhaustive investigation of putative exceptions to the democratic peace by asking whether wars between democracies are merely rare, as many cautiously contend, or are actually nonexistent. It should be noted that some of the arguments and conclusions of these two chapters appear in extended and revised form from Ray's previously published articles. The third investigative track occupying chapters 4 and 5 reports new findings from a pair of highly structured comparative case studies of the Fashoda Crisis and the Spanish-American War. A final chapter summarizes the main results and offers some observations regarding the future of the democratic peace.

Ray's initial chapter provides an illuminating road map to the democratic peace literature, extending from its early philosophical roots in Kant to the most recent empirical studies. Although readers will have little doubt where Ray's own sentiments lie, his treatment of the contemporary debate scrutinizes the arguments of both sides with an equally critical eye. But this opening essay is much more than the standard rehearsal of published literature, for it also presents a novel reinterpretation of conventional wisdom on the democratic peace. Perhaps no proposition is more firmly embedded in this conventional orthodoxy than the associated claim that democracies are generally no more peaceful than other types of states. Ray confronts this proposition head-on with enough literature ignored or overlooked by others to leave even skeptical readers wishing for more definitive research on the matter. At another point Ray departs from most other democratic peace theorists by arguing that the democratic peace proposition is not at all incompatible with conventional realist theory. Readers who remain unconvinced may find his case turning more on his particular interpretation of realism than on anything having to do with democracy. But perhaps most intriguing is Ray's use of Hegelian philosophy in considering the potential global implications of the democratic peace.

Although succeeding chapters address a somewhat eclectic collection of topics, Ray skillfully manages to weave them together into a single fabric. For example, early on he concedes that the democratic peace may indeed be vulnerable to the charge that wars between democracies are rarely observed in history simply because both democracies and wars are themselves so exceedingly rare. Yet, this concession is a strategic one, for once made it immediately elevates the significance of any possible exceptions to the strict claim that democracies have never waged war against one another. Ray then considers a long list of reported exceptions, all of which fail to qualify as wars or democracies (or both) under his carefully stipulated definitions. Definitions are the crux of the matter, of course, and while Ray admittedly crafts his definitions to produce the desired outcome, the definitions he devises are in the end neither strained nor unreasonable.

The case studies in chapter 5 are also tied to the statistical rarity of wars and democracies, albeit in a somewhat different manner. It is precisely because democratic wars are not very likely under any circumstances that aggregate statistical evidence must be supplemented "with insights and persuasive power that can be generated only by intensive analyses of individual cases" (p. 207). The cases under analysis have been cleverly chosen to form a most similar systems comparison of two international disputes from 1898, one involving two democracies (France and Britain) that narrowly avoided war (Fashoda) and the other involving a democracy (United States) and a marginally nondemocratic opponent (Spain) that did result in war. Ray structures his comparison as a "pseudo-experiment" in order to fix attention on the numerous factors other than democracy that might plausibly account for the differing outcomes. There is much to admire in Ray's deft handling of his two cases. Here, as elsewhere, he carefully considers all sides of the available historical evidence and prudently qualifies his assertions where necessary. This chapter also stands as a methodological exemplar of how case studies and statistical findings can mutually reinforce each other.

Not every chapter is quite so tightly integrated to the theme, however. The most transparent example is chapter 4, which is devoted entirely to the methodological justification of case studies. Few will quarrel with Ray's treatment of this issue—it is a superbly integrated statement of the widely held view that judiciously constructed case studies and statistical analyses are complementary. In fact, this chapter stands virtually on its own and could well serve as a valuable methodological resource for students and others employing case studies in their own research. Still, one wonders whether so extensive a treatment was really necessary in this particular context.

Questions of relevance may also arise with regard to Ray's analysis of regime transitions (chap. 2). Here Ray employs an innovative procedure to disentangle state-level from systemic-level effects on global regime changes. The main point about regime change is plain enough: If democracy implies peaceful relations with other democracies and if more states are turning toward democracy, then the world should become a more peaceful place. This is indeed a profound statement, with implications of immense proportions, which is obviously relevant to the democratic peace and surely is one reason why the issue has so captured the attention of scholars and policymakers alike. What is not quite so plain, at least in the context of the democratic peace, is why it matters whether regime transitions are governed by system-level or state-level forces. Ray seems to be suggesting that the global ramifications of the democratic peace are on a more precarious footing if state-level factors dominate. Yet, why this must be so is never made entirely clear.

This is a book with something of genuine value for anyone seriously interested in democracy or international conflict and should be essential reading for those conversant in the contemporary debate over the democratic peace. The questions Ray addresses are among the most important of our time, and, while he does not provide all the answers, he does offer us many fresh insights and new ways of thinking intelligently about them. Those who have already made up their minds on the democratic peace may well find certain of his arguments discomforting, but in the end this is a small price to pay for the book's rich rewards.

**Jacques Delors and European Integration.** By George Ross. New York: Oxford University Press, 1995. 326p. $35.00.

Leon Hurwitz, *Cleveland State University*

Five hundred years after the Europeans discovered and began to form a New World in the Americas, they have set out to rediscover and reorganize their Old World at home. The 1957 Treaty of Rome, building upon the 1951 Treaty of Paris (the European Coal and Steel Community), established the European Economic Community (the predecessor of the European Community/European Union). As refined by subsequent treaties (most notably the Single European Act of 1987 and the Treaty on European Union [the "Maastricht Treaty"]) and combined with the current intergovernmental conference, this initiative shows that Western Europe represents the best example of regional interdependence, the enhancement of the management of international cooperation, and the harmonization of certain public policies. Although there have been many management failures in the past, it is the European Community/European Union that has enjoyed the most success in regional economic—and political—integration and the management of international cooperation.

George Ross's study of Jacques Delors and European integration could not have appeared at a more timely moment. The intergovernmental conference, with its year-long deliberations over the operationalization of the Maastricht Treaty, opened in Turin in late March 1996. The final decisions reached at this conference will no doubt be a surrogate measure of Jacques Delors's legacy.

The book is a rather detailed account of the author's six-month sojourn as a "participant observer" in the cabinet of European Commission President Jacques Delors at (the now nonfunctional) Berlaymont in Brussels and places elsewhere during the first half of 1991. Unfortunately, and the following comments are the only negative ones offered, Professor Ross appears to have fallen prey to the flaw endemic to this methodology: the participant-observer arrives home with so much material that it is usually very difficult or even impossible to discard even one notecard or cassette. The trivial for the outsider becomes the significant for the researcher; the amusing but unnecessary anecdote becomes empirical analysis; the mundane becomes a matter of state and high politics. It is as if the researcher believes that if seen (or heard or recorded or thought), the information must therefore be published.

This book does not escape this criticism; a few examples will suffice: people "bark" orders; the seating arrangements around a table are described along with the wine service;

states of mind and motives are imputed; lunches are characterized as "gloomy" (the food? the people? the conversation? the bill?); even the weather does not go unnoticed (it was a "cold and foggy" day). Perhaps the best (worst) example of this occurs when Professor Ross describes some intellectual (or lack thereof), physical, and personal characteristics of some former commission members: "Manuel Marin, the Commission matinee idol (he was a young and handsome man), was unpredictable and moody" (p. 160). Would that participants refrain from publishing such observations!

These comments aside, the book is a fascinating treasure for those among us who care about international integration, the European Union, decision-making processes, and the individuals who are involved. Although Professor Ross is quite respectful of Jacques Delors, the book is not hagiography—some of the warts and pimples are revealed. The author writes that Jacques Delors "is the most successful Commission leader in the history of the Community" (pp. 14–15). I agree, but I would go one step beyond George Ross: Jacques Delors's success transcends the commission, and he will join the select group of those who rediscovered Europe (e.g., Jean Monnet, Robert Schuman, Konrad Adenauer, Walter Hallstein, Paul-Henri Spaak, Maurice Faure, Gaetano Martino, Joseph Bech, and J. W. Beyen).

Jacques Delors arrived at the European Commission at a time of "Euro-pessimism" and "Euro-sclerosis" but through several bold initiatives (e.g., the Single European Act, Maastricht, the Common Security and Foreign Policy, enlargement, European Monetary Union) he reinvigorated the process through economic liberalism while maintaining and strengthening Europe's social uniqueness. The union that Jacques Delors left to his successors is different—vastly different—from the one he found in the mid-1980s: a union of 15 with as many as 25 additional countries camped on Brussels's doorstep.

There still remains a host of unresolved issues—issues that the 1996–97 Intergovernmental Conference has on its agenda: widening versus deepening; relations with the rest of Europe; the Common Security and Foreign Policy; a new institutional framework; voting procedures; common or harmonized policies regarding police cooperation, immigration, taxes, and welfare; European Monetary Union with its "Euro-Fed" and a single currency; the democratic deficit.

But perhaps even more crucial to the future of Europe than these issues and having a link to how the future will assess the contributions of Jacques Delors is the very nature that the integration process will assume. There are at least three models:

(1) Europe "a la carte"—each member state can pick and choose the specific public policies to which it will adhere and from which it will opt out (or, as the British claim with monetary union, the ones it will opt into). This model by its very nature denies the longstanding tradition of accepting the *acquis communautaire*.

(2) Europe "at different speeds"—certain core countries will be the vanguard of achieving new levels of integration (e.g., the monetary union and a single currency) with the laggards slowly, but eventually, catching up.

(3) Europe "on a bicycle"—all 15 (or 20 or 30 or 40) must pedal together at the same speed in a forward direction or the entire infrastructure will crash.

George Ross writes (p. 16) that Jacques Delors was an aficionado of cycling in his youth.

**Towards a New Europe: Stops and Starts in Regional Integration.** Edited by Gerald Schneider, Patricia Weitsman, and Thomas Bernauer. Westport, CT: Praeger, 1995. 240p. $55.00.

Robert Leonardi, *London School of Economics*

This book tries to break new ground in the study of the European Union (EU). In the past, studies of the EU have either been extremely descriptive in nature (based on the assumption that the reader knew nothing about European integration) or bogged down in the debate between intergovernmentalists and comparativists over how the EU should be studied. The contributions contained in *Towards a New Europe* break through this intellectual logjam by advocating and showing how the study of the politics and policies of the EU can be grounded in more rigorous, mathematical formulations of spatial models. As the editors state in the concluding chapter, the study of the EU needs to be placed on more rigorous research ground if we are ever to move toward a future when research will become cumulative and the assumptions behind the research will be rendered explicit.

The authors have done a superb job in illustrating the types of models that can be used to study the decision-making process associated with the issues of expansion of members as the European Union contemplates its future after the year 2000 (the Cederman, Pahre, and Mattli chapters), the role in the overall decision-making process in which the European Parliament plays an increasingly important part (Tsebelis), and the logic associated with the future selection of Europe's central bankers (Martin). These chapters are combined with two chapters dealing with issues tangential, but complementary, to the study of the EU: the nature of Swiss federalism ("Switzerland—Still a Paradigmatic Case?" by Hug and Sciarini) and the future of NATO ("Full Membership or Full Club? Expansion of NATO and the Future Security Organization of Europe" by Bernauer).

Despite the innovative approach, the book is less successful in producing genuinely new research results. It is not clear how the models proposed can be used, above and beyond the few cases and the limited period cited in the chapters. What can the proposed models tell us about other periods when decisions at the European level were taken under a different set of rules and when the alternatives available to national governments in the realm of sectoral policies were based on a significantly different set of assumptions? A model is of value when it allows us to understand a wide variety of cases, rather than only the few cases discussed in these chapters.

In reading the book I was reminded of the vast literature on modeling in the field of econometrics and evaluation studies. The theoretical work is illuminating in its formal, mathematical structure, but it is much less satisfactory when one tries to use the models to understand the empirical reality. Either the models are so complex that they take into account everything (thereby making it difficult to elaborate parsimonious conclusions) or the operationalization of the models is so narrow that very little is added to our knowledge of empirical reality.

The lack of a thorough empirical testing of the models proposed here is one of the shortcomings of the book. Scholars of the EU will pick up this book with the expectation of finding answers to a series of important questions, but they will find few of them in these contributions. The shortcomings in the volume are associated with the following:

(1) The assumptions made by some of the authors in formulating their models and the empirical evidence assembled are mostly quite limited in nature and not at all satisfactory in creating the basis for the formulation of robust theories in explaining postwar evolutions in Europe. The assumptions tend not to take into full account the complexities of overall decision making in the EU and the interactions among different levels of decision making.
(2) There is little sensitivity to the changing logic and decision-making structures that have evolved over time as the Union has moved from one treaty to the next. The models appear to be detached from time, space, and institutional setting.
(3) The number of feedback mechanisms found in some models make it difficult to formulate any clear cause-effect relationship capable of being tested in the empirical world.
(4) The rational choice approach underlying these models still begs the question concerning the application of theory or theories to understanding the dynamics of European integration.

What will be found in the various chapters is a series of thought-provoking hypotheses on decision making within the EU that can be actively pursued by more empirically oriented researchers. A positive aspect of the book is that it does not marry any particular view of European integration. The contributions are not Euro-skeptical or Euro-euphoric in nature. The questions posed in each chapter are objective, and the conclusions can be interpreted from a variety of perspectives.

The book would have benefited greatly from a longer introduction and a more thorough evaluation in the concluding chapter of the proposed models in analyzing the course of European integration. The introductory chapter would have benefited from an analysis of other approaches to the study of the EU and its evolving goals and institutional architecture. Finally, the concluding chapter should have spelled out in greater detail the contributions made by the various authors and what still needed to be done to broaden and deepen our understanding of the dynamics of the European Union.

**U.S. Civil-Military Relations: In Crisis or Transition?** Edited by Don M. Snider and Miranda A. Carlton-Carew. Washington, D.C.: Center for Strategic and International Studies, 1995. 224p. $18.95 paper.

Sam C. Sarkesian, *Loyola University Chicago*

The collapse of the Berlin Wall and the unraveling of the Soviet Empire created a new environment for international politics and considerably reduced the fear of major wars between major powers. This has led to numerous studies, reports, and commentaries by a number of those in American political and military circles on how to reshape the U.S. military for the new era. These range from the Bottom-Up Review to designs for a twenty-first-century military and efforts to characterize the types of wars or conflicts in which American armed forces might become involved. All of this is complicated by the revolution in military affairs. Some of the literature examines socially relevant issues shaping the military, such as women in combat roles, homosexuals and the military, demographic changes, and quality of family life in the military. As important as many of these studies are, they tend to overshadow and ignore civil-military relations. And the few studies that do give primary attention to civil-military relations often do so from a narrow perspective.

This volume edited by Snider and Carlton-Carew does much to fill this gap in the literature. The book examines most of the major dimensions of civil-military relations without imposing a particular conceptual dogma on the analyses. It evolved from a conference on civil-military relations in September 1994 organized by the Political-Military Studies Program at the Center for Strategic and International Studies and supported by the Strategic Studies Institute (SSI) of the U.S. Army War College. The volume consists primarily of papers presented at that conference by an impressive list of scholars and analysts representing a variety of disciplines and perspectives. From these, the editors have produced a book that includes chapters by James F. McIsaac and Naomi Verdugo, Mark J. Eitelberg and Roger D. Little, Curtis L. Gilroy, Stephen J. Cimbala, Peter D. Feaver, Paul Bracken, Michael C. Desch, David R. Segal, and Douglas Johnson and Steven Metz.

The introductory chapter by the editors provides broad theoretical and conceptual directions for the book. It includes summaries of prominent theories on civil-military relations such as those of Samuel Huntington, Morris Janowitz, and Charles Moskos. Using the framework and directions outlined in the Introduction, the remaining chapters examine various aspects of civil-military relations.

What emerges is wide-ranging analyses of important facets of civil-military relations. This is seen even in a brief sampling of themes. One theme notes that during and since the 1992 presidential elections the military became more prominent in civil-military relations, and one suggestion is that the balance shifted in favor of the military. President Clinton's difficult relationship is examined as part of the domestic view of civil-military relations. Other themes point to the Cold War as a watershed in the relationship between the military and various elites. Also discussed are a number of ways to restructure the military for the new international order. Some point to the Goldwater-Nichols reform act and the role of General Colin Powell as harbingers of a more prominent military role in society. In the last part of the book, the civil-military literature is reviewed with a final assessment pointing to important trends that need continual study, ranging from changes in the nature of armed conflict to sociocultural and normative changes.

Even with such wide-ranging perspectives, the general themes of the book can be grouped into four questions:

(1) Are civil-military relations in crisis?
(2) What is the meaning of civil-military relations in the post-Cold War environment?
(3) What is the relationship of the military to a variety of political-social elites, including the president and the media?
(4) What does all of this mean for the remainder of this century and into the next?

While these questions are not necessarily addressed explicitly, their currents underpin the book. That there are changes taking place in civil-military relations is the clear message to be gained from the various chapters. But there is no single set of answers, nor is there any consensus on the scope of the questions or on the definition and concepts of civil-military relations. Perhaps a lack of resolution is to be expected, given the complexities of the subjects, the various theoretical approaches, and the ill-defined strategic landscape. More specifically, there is no general agreement on the meaning of "balance" in civil-military relations, whether civil-military relations are in crisis, or what impact the strategic landscape and operations other than war have on civil-military relations. There is general agreement only on the need for continuing study of civil-military relations.

The importance of the book and its major contribution to the literature are in the range of assessments and the variety of approaches. These provide rich analytical dimensions, stimulating analyses, and conceptual breadth. Even with the excellence of this book, however, there are two areas that deserve more attention. First, little attention is given to the National Guard and the federal reserves. The close relationship between those in the reserves and their civilian base may have some impact on the active military and the character of civil-military relations. Second, more attention should be given to the fact that the military is not a monolith. To be sure, it is important to deal with the military as an autonomous entity in broad policy and strategic dimensions. But it may also be important to identify and define the various aspects of civil-military relations that evolve from differences among the military services and, indeed, differences within each service.

These concerns aside, this is an important book and essential reading for those dealing with military issues. It is especially important for political leaders who are trying to design defense budgets to shape a military system that can effectively adapt to the new international landscape. The book is also important reading for those who want to commit the military to a variety of domestic missions and for those concerned about how civil-military relations will be shaped by the rise of a new generation of political leaders, most of whom have no real-life military experience. Finally, the book holds important insights for general readers concerned about the future directions of U.S. national security policy and the shape of the American armed forces.

**The Nixon Administration and the Making of U.S. Nuclear Strategy.** By Terry Terriff. Ithaca: Cornell University Press, 1995. 252p. $35.00.

Eric Mlyn, *University of North Carolina at Chapel Hill*

Terry Terriff has taken the analytic equivalent of a microscope to innovations in U.S. nuclear weapons policy undertaken from 1969–74. The strength of the book is Terriff's complete mastery of this period and his nuanced and sophisticated presentation of deterrence theory and how its various parts influenced U.S. nuclear policy. But the strength of the detail creates significant problems for the book, which in the end fails to link this narrow and detailed case study to broader theoretical, historical, or substantive issues. As a result, the book may be of interest to those who are already expert in the field of nuclear studies, but it is unlikely to engage others with an interest in either the history of the Cold War or theories of foreign policy and international relations.

In describing changes to U.S. nuclear weapons policy, Terriff's book is much in the tradition of Janne Nolan's (1989) *Guardians of the Arsenal*, Scott Sagan's (1989) *Moving Targets*, and Fred Kaplan's (1983) *Wizards of Armageddon*. Unlike these other books, however, Terriff's work does not present a comprehensive account of U.S. nuclear weapons policy. His goal is to describe a five-year period and to show that the reasons given in public for changes to U.S. nuclear weapons policy during the Nixon administration were different from the real forces that went into changing that policy.

Most who have studied the history of U.S. nuclear weapons policy have looked to the conflict of U.S. and Soviet interests in Europe as the primary fulcrum to explain changes in nuclear policy. The history is well enough known by now. The nuclear age saw U.S. policymakers espouse a policy of Mutual Assured Destruction (MAD), which indicated that policymakers did not believe that nuclear weapons could be used in a rational, limited way. At the same time that they were advocating MAD, their nuclear targeting moved in the direction of Nuclear Utilization Theory (NUTS), a policy that planned and targeted nuclear weapons in a way that would enable the United States to fight and win a limited nuclear war.

And this is where Terriff's story comes in, as he carefully traces one in a series of attempts by high-level policymakers to add flexibility and accuracy to the U.S. nuclear arsenal. Terriff tells the story of the period of 1969–74 but fails to link this to the fact that policy had been evolving this way since the early 1960s and would continue to evolve in this direction up through the Bush administration. Although Terriff rightly notes that the changes during this period were further pursued during the Carter administration, he fails to place this period in its proper historical context.

Central to this book's narrative is the role of National Security Adviser Henry Kissinger and the usurpation of his leadership on nuclear issues during the Nixon administration by Secretary of Defense James Schlesinger. According to Terriff, Kissinger favored an enhancement of U.S. conventional capabilities to resist Soviet probes in the Third World. Nonetheless, Kissinger was sensitive to domestic political pressures to reduce defense spending and recognized that a conventional buildup was simply not possible. However, because of his concerns after the 1970 Jordanian Crisis, the India-Pakistan conflict of 1971 and the ongoing Sino-Soviet rift, Kissinger desired military plans that would allow the United States to deter and directly counter Soviet probes across the globe. Since a conventional buildup was not possible and U.S. strategic policy did not contain limited, discriminating options, Kissinger began a process that he hoped would give the president usable nuclear options in a variety of Third World contingencies. Terriff is mostly convincing in this account, and this is an important contribution because it counters the widely held belief that changes to U.S. nuclear weapons policy were always propelled by U.S. policymakers' concerns about the credibility of extended deterrence in Europe.

Also interesting is Terriff's account of how Secretary of Defense James Schlesinger claimed credit for the Schlesinger Doctrine, whereas the planning for the doctrine, which was finally announced in January 1974, had been going on for five years and just as easily and perhaps more appropriately could have been called the "Kissinger Doctrine." Schlesinger's spontaneous announcement of this long-planned shift was also, according to Terriff, confusing because he justified the new refined options according to reasons that were not primary for those who had designed the policy.

Schlesinger said in spontaneous comments that the changes were needed to recouple U.S. strategic nuclear weapons policy to extended deterrence in Europe and to reinvigorate U.S. counterforce efforts in order to counter Soviet counterforce capabilities. Although this is an interesting shift, Terriff fails to convince the reader of its importance, and since this is one of the major points of the book, the reader needs to be convinced that in fact this difference matters. After all, Terriff is not arguing that it was not strategy that drove U.S. nuclear weapons policy (this would indeed be an interesting argument), only that the strategic concerns were other than those that were publicly announced by Schlesinger. Furthermore, in the scheme of things, the difference between the regional concerns of Kissinger and those publicly announced by Schlesinger were one of degree, something Terriff himself acknowledges. "In propagating these rationales for innovating U.S. strategic nuclear doctrine, Schlesinger did not compromise the basic principles of the guidance policy. Nor was his presentation entirely misleading, for the new policy could serve these ends. What Schlesinger did, rather, was recast an essentially technical policy document so that the declaratory statements of policy conformed with current political realities and furthered the political objectives of the United States" (p. 202). In addition, in his chapter on the implementation of the nuclear guidance Terriff does not directly link Schlesinger's public justification of the policy to the inability of military officials to translate accurately civilian guidance. The overall significance of the central substantive account in this work is left to the reader to determine.

Terriff's account is important for profoundly important questions it raises and disappointing in that the author leaves most of these questions unexplored. These questions revolve around three important literatures within political science and international relations. First, there is a large literature that carefully explores the decision-making process and its impact on nuclear weapons policy. However, though Terriff briefly mentions the impact of bureaucratic and domestic politics, the book does not substantially contribute to this literature and in fact pays very little attention to it. Second, Terriff's account does not look enough at what came before 1969 or after 1974 to place this case study in its historical context. Such an effort would help readers recognize that this account is one of continuity rather than change, an important historical point. This failure to establish a historical context is crucial since it suggests that there is nothing unique about the time period that Terriff has studied.

Finally, Terriff's findings are also relevant to current consideration of changes to U.S. nuclear weapons policy. It is worth noting that though the times are very different, policymakers today find themselves with serious constraints on the defense budget, while U.S. interests abroad are extensive. These are precisely the same circumstances that led policymakers in the late 1960s and early 1970s to rely more heavily on nuclear weapons. And although the Cold War is over and the United States and Russia have in fact reduced the size of their nuclear arsenals, the United States has not significantly shifted away from the type of policy that Terriff describes. Some consideration of this point would have made this book of interest to a wider audience. As the book stands, Terriff has provided us with a competent though highly circumscribed account of a small part of U.S. nuclear history.

**Free Markets and Food Riots: The Politics of Global Adjustment.** By John Walton and David Seddon. Cambridge, MA: Blackwell, 1994. 387p. $57.95 cloth, $23.95 paper.

Joan M. Nelson, *Overseas Development Council*

Since the 1970s, most of what used to be called the Second and Third Worlds has seen rapid integration into international markets (often with exploding debt), growing external pressures for economic stabilization and liberalization, high social costs, episodic urban protest, and substantial (though uneven and fragile) political opening. This book explores links among these trends, focusing on causes and effects of protest.

Regarding causes, the introductory section offers a challenging overarching thesis, drawing a parallel between eighteenth-century riots in Western Europe and contemporary anti-austerity protests. Eighteenth-century riots were often triggered by sharp rises in food prices. Studies of the timing, location, participants, and goals of these protests suggest far broader underlying causes. Emerging integrated national markets dismantled myriad local trade restrictions and protections. The rise of strong central states reduced local authority and control over forces affecting the welfare of the people. Market liberalization and centralized power dissolved the social contract between peasants and local authorities, which had guaranteed adequate supplies of food at affordable prices in exchange for political quiescence. Protests were not simply reactions to hardship but were expressions of moral outrage, reflecting basic shifts in community life and the relation between state and economy.

Similarly, the authors argue, in the current era trade and price controls and subsidies are being dismantled to create more open and efficient economies within an integrated global economy. Like eighteenth-century local authorities vis-à-vis strengthened national states, governments of developing and post-communist nations now find impersonal supranational forces and all-too-concrete international agencies constrain their policies and actions. Urban working- and middle-class groups see their wages, subsidies, and services eroded by austerity, market liberalization, and privatization, and their implicit social contract with political elites betrayed.

This overarching thesis offers an instructive perspective on contemporary protest. It also raises broader and more provocative but unaddressed questions. The historic integration of national markets and the industrial revolution caused widespread suffering. But despite a considerable time lag, they generated undreamed-of prosperity for ordinary people. In hindsight, many social costs could have been reduced. For our times, the burning issues are two: (1) Will globalization also ultimately generate far wider prosperity? and (2) Can the costs be eased?

The geographic chapters that make up most of the book retreat to narrower perspectives. They provide concise, information-packed reviews of economic trends and protest patterns in four regions, as well as India and Sri Lanka. Yet, they do not assess the trends from the perspective suggested by the historical analogy. They emphasize the corroding effects of international integration (above all, debt), the power of international financial institutions, and the erosion of implicit social compacts between urban middle and working classes and ruling elites. Internal weaknesses are barely mentioned. Yet, by the late 1970s some in Latin America and Eastern Europe were already arguing that import-substitution and central-planning models were exhausted. The descriptions in these chapters of patronage, protection, price controls, and subsidies gloss over the fact that (save in Eastern Europe) the benefits of these went overwhelmingly to the wealthy and the middle strata (including organized labor), doing little for the informal urban economy and nothing for the mass of rural poor. The chapters emphasize the social costs of price increases coming after devaluations demanded by the IMF, but are silent on the tremendous direct and indirect costs, above all to the poor, of cyclic high inflation or hyperinflation long predating structural adjustment programs. Nor do they note that even greater hardship might result from failure to contain government deficits, correct unrealistic exchange rates, and begin to address severe institutional problems.

The regional chapters are pessimistic concerning economic prospects. Yet, by 1992 determined stabilization and radical market-oriented reforms had already spurred 9% growth in Argentina; within the next year Peru, Poland, and the Czech Republic turned the corner. The transformations under way are costly and uncertain, but—like the integration of national economies and the emerging industrial revolution some two centuries earlier—gains are also emerging (still skewed toward the wealthy, but spreading downward).

The regional chapters and the overarching thesis together offer valuable insights into the nature of protestors' grievances. But perhaps unintentionally, they encourage the unwary reader to adopt the protestors' perspective, viewing hardships as externally imposed and seeking solutions in restoration rather than transformation. That perspective tremendously oversimplifies the complex, unpredictable trends under way and their immense potential for both injury and progress.

*Free Markets and Food Riots* is concerned not only with the underlying causes of protest but also with its consequences, particularly for the emergence of democracy. Some of the suggested links seem questionable. Surging nationalism and ethnic tensions in the post-communist world undoubtedly are aggravated by economic hardship but are an inevitable outcome of the disintegration of the Soviet empire and the Soviet Union. The alleged link in India between market reforms and militant Hinduism is also dubious; surely, any such link would be mainly a product of specific political exigencies rather than any intrinsic or durable connection. More central and convincing arguments focus on the interplay among emerging grassroots social movements (in which women often play a prominent role), social protest, and political opposition. In some cases, such as Ghana and Benin, economic protest spurred political liberalization. The chapter on women's roles in protest paints a continuum spanning forms of "everyday resistance" and coping mechanisms, community organization and protest, and larger political movements. Protest against austerity blends with opposition to corrupt, oppressive government.

Again, a broader comparative perspective would have enriched an insightful analysis. Actual or promised economic reforms can be popular: witness recent elections in Argentina, Peru, and Brazil. Recent left-leaning elections in Eastern Europe reflect yearning for less pain but not rejection of market transformation. Protest is but one element in a complex array of political responses to bewildering transition. The challenge is to understand what generates different prevailing reactions.

**Human Rights and Reform: Changing the Face of North African Politics.** By Susan E. Waltz. Berkeley: University of California Press, 1995. 295p. $40.00 cloth, $16.00 paper.

Lisa Anderson, *Columbia University*

This is both an ambitious and a modest book. It is ambitious in the variety of audiences it seeks to address and modest in the claims it seeks to make to each of them. Let us take the ambitions first.

This book could be—and, indeed, should be—marketed as a textbook on modern North African (or at least Moroccan, Algerian, and Tunisian) history and politics, suitable for classroom use. It provides useful surveys of the nineteenth- and twentieth-century histories of the three countries and easily the most up-to-date treatment of their recent political travails currently available in English in one volume. Waltz provides particularly useful versions of the increasingly widespread revisionist interpretations of nationalist histories, notably the greater emphasis on the traditionalist challenge to Tunisian President Habib Bourguiba in the 1950s.

This book is also a study of the growth of human rights advocacy and mobilization as an example, or type, of social movement. Arguing that North African society was traditionally patrimonial and patriarchal, by which she intends to suggest not only male domination but "a system of authority relations wherein all individuals learn to submit their own personal identity to that of the group—that is, the family" (p. 41), Waltz traces the embrace of human rights conceptions and the growth of organizations dedicated to their promotion to the decay of patriarchy and consequent social and cultural dislocation in the region. As such, the book is a provocative "prematerialist" extension of postmaterialist explanations of value change and new social movements and might be assigned as recommended reading in a course on social movements or on ideas and politics.

Finally, this is also an advocate's book. Waltz is not only a specialist on Maghribi politics but a longtime Amnesty International activist, and she knows most of the actors in her story personally. This leads her to interpretations that are open to debate—my personal view of the murky world of Algerian human rights advocacy is more sympathetic to Abdennour Ali Yahia and less tolerant of Miloud Brahimi—but that are never uninformed and are often unusually knowledgeable.

Ironically, it is precisely these several varied ambitions that ultimately contribute to the modesty of the book as a whole. It is clear, accessible, well organized, but almost too decorous, too concerned to provide adequate historical background or to demonstrate theoretical evenhandedness. In the end, the message is diluted, the punch pulled.

For a textbook on modern North African history and politics, the human rights lens provides little purchase on some crucial developments that fall outside its purview—economic development, population growth, technological change. The story Waltz tells in her 100-page "North African Political Context" section is good for what it is, but there is a lot one might wish for in a text on the history and politics of North Africa that it is not.

Similarly, although Waltz announces early on that she will argue that it is the decline of the patriarchal order that "has fueled both the human rights and the Islamist movements across the Maghrib" (p. 36), she soon loses the train of her argument. Her careful description of the "context" interrupts and ultimately takes the place of sustained analysis. Implicitly she is contrasting what she sees as an Islamist effort at restoration of the threatened patriarchy to the entirely different conception of societal organization embodied in international notions of human rights, based as they are on nineteenth-century European liberal individualism. Yet, she devotes little attention to the extent to which the individualism of liberal human rights contradicts the familial or household loyalties that undergird the social and cultural patterns she characterizes as patrimonial.

This prevents her from fully accounting for the limits and failures of the human rights movements in the region. She describes—particularly in a sad and powerful epilogue that begins with the assassination of Youcef Fethallah, the president of Algeria's principal human rights organization in June 1994—the process by which human rights concerns were overtaken and submerged in elemental contests between governments and Islamists, both of whom disdain liberal conceptions of human rights. She does not, however, explain the nature of that disdain, which represents the failure of the values that most of the audience for this book would take as self-evident to win widespread support.

Obviously, governments accustomed to unlimited discretion and risking embarrassment prefer not to be monitored by human rights advocates or anyone else. But why has human rights advocacy remained a pastime of the elite, while the masses react to social dislocation, economic deprivation, and political repression by taking solace in an Islamist opposition? Perhaps if, as Waltz suggests, the primacy accorded by patrimonialism to familial obligations and identities inhibits recognition of the individual autonomy upon which liberal theory is built, human rights advocacy is precisely as revolutionary (and perhaps, too, as "culturally imperialist") as its opponents insist.

Waltz's argument has important implications, if I have understood it correctly, for both analysis and policy. As an analytical matter, in the face of social and cultural norms that give such high priority to group—in this instance, familial—identity, the utility of assuming the self-interested individual as the irreducible analytical unit should probably be treated as an empirical question. On the policy front, human rights advocacy has a two-pronged mission: not only to constrain the arbitrary and capricious actions of governments but also to foster the conception of the individual to which adhere the rights that limit government.

These are not simple questions, in science or in politics, and Waltz can hardly be faulted for having failed to resolve them all in this volume. Although she might have drawn out the implications of her own analysis in a somewhat more sustained argument, she is still to be congratulated for having provided a fair, clear, and responsible place from which to begin.

**America and the Persian Gulf: The Third Party Dimension in World Politics.** Steve A. Yetiv. Westport, CT: Praeger, 1995. 192p. $49.95.

K. A. Beyoghlow, *Marine Corps Command and Staff College*

This book advances the central thesis that conflict between two antagonists (in this case, between actors in the Persian Gulf region) has had consequences for third parties in unintended ways. The cornerstone of the argument is that the United States as a third party to many recent conflicts in the Gulf region has benefited tremendously from such outcomes as the Iran-Iraq War, the Soviet involvement in Afghanistan, and the Iraqi invasion of Kuwait; that is to say, the power of the United States increased in proportion to the loss of power by Iran, Iraq, the former Soviet Union, and others. According to the author, Iran, Iraq, and the Soviets all "missed significant foreign policy opportunities to undermine U.S. regional security and to strengthen their own position" (p. 138). The author goes on to argue that, unlike other studies of conflict and war, his book is about effects, not causes. He considers this approach to be unique and different from other theories of international relations.

The book is divided into eight chapters: "Conflict and Consequences in World Politics"; "Statecraft, History, and Third Party"; "The United States and the Iranian Revolution"; "Moscow Plays into U.S. Hands"; "The Chain of History: Linking Regional Wars"; "The Middle East after the Storm"; "Missed Opportunities"; and a conclusion.

Although rich in both data and analysis, the book raises more questions than it answers. It contains sweeping generalizations and comes across as a hodgepodge of concepts and ideas that still need to be tied or compared more rigorously to a broader set of theories on conflict and war. One gets the impression from reading this book that the United States was a passive bystander in all of the above conflicts and was able simply to reap handsome profits when each conflict had run its course. The empirical reality is that the United States was a prominent player in all of these conflicts, in fact, directly and indirectly supported efforts to defeat the Soviets in Afghanistan, worked relentlessly to counter Iran and to shore up Iraq until the time the latter invaded Kuwait, and arm-twisted its friends and allies—as well as traditional foes (Syria)—into joining a coalition against Iraq. The United States was the main antagonist in the Gulf War; Iraq the main aggressor.

While Professor Yetiv's approach is thought provoking, it fails to recognize the crux of the problem, namely, that the consequences of the conflicts under consideration are better suited to a multidimensional treatment related to the Cold War and post–Cold War plans and objectives. Much can also be said of other frameworks, especially that of Keohane and Nye (*Power and Interdependence* [1988]). I believe that the author's classification of the United States as a "third party" is inherently problematic. Other, more appropriate, regional players, such as Pakistan (in the case of Afghanistan), the Gulf Cooperation Council (in the case of the Iran-Iraq War), and Saudi Arabia and Iran (in the case of the Iraqi invasion of Kuwait), would better fit that description. In other words, instead of focusing on the increasing security fortunes of the United States, the third-party dimension may be more amenable to a better understanding of effects related to intra- as well as interstate conflict (*intra* in the form of political alienation and violence; *inter* in the form of lack of cooperation on salient security issues that affect all regional actors). Cost-benefit theories such as those connected with the Cold War, the post–Cold War, power and interdependence, hegemonic leadership (Keohane), rational actor (Allison), bipolarity (Waltz), and functionalism/neofunctionalism (Haas) are more useful here.

It is also difficult to measure U.S. prestige as an outcome of the above conflicts. U.S. security in the Gulf remains precarious at best. The United States failed to fulfill a critical objective (following the defeat of Iraq) of establishing a viable security structure and opted instead to continue the status quo of bilateral security relations. Dual containment has not proven to be an unqualified success in creating the security architecture so desperately needed in this part of the world. Some important U.S. allies in Europe and in the Gulf have had second thoughts about the viability of dual containment. They want to play, but the United States is reluctant to initiate a change in its policy. The Gulf is likely to remain a security burden for the United States, and the United States must not lose sight of the fact that its increasing power will not be fully realized without negative effects.

Finally, within a rapidly changing global environment, particularly in the field of information warfare and technological innovation, the superior power of the United States has had dramatic effects on both allies and foes. This "soft" power is key to explaining outcomes of future regional conflicts, particularly as global crises subside and regional ones proliferate. This trend that began in the early 1980s is related to an increasing natural tendency by the United States to move toward imperial hegemony rather than being a third-party accidental benefit. Can the United States continue to sustain such a hegemonic role in a world that badly wants to be multilateral? Can the United States maintain its current security relationships and continue to successfully sustain its forward presence in the Gulf and beyond? The real paradox here is one of maintaining credibility and guarantees at the same time.

These questions and issues have been prompted by this book, which opens up a new debate about the future of American power. It is primarily in this specific sense that the author has made a significant contribution to the fields of international relations, national security, and regional studies.

**Elusive Peace: Negotiating an End to Civil Wars.** Edited by I. William Zartman. Washington, D.C.: Brookings Institution, 1995. 353p. $42.95 cloth, $18.95 paper.

Thomas G. Weiss, *Brown University*

A diverse group of experts in area studies and negotiation theory have contributed to this first-rate compendium of 11 case studies (Sri Lanka, Basque Spain, the Sudan, Eritrea, Lebanon, South Africa, Angola, Mozambique, Afghanistan, Colombia, and the Philippines). The inverse relationship between diminishing East-West tensions and increasing civil wars, the primary challenge to international security in the post-Cold War era, animates much contemporary political analysis and this book.

A longer historical view—many of the cases pre-date the end of superpower confrontation and have taken on lives of their own—is helpful in that out-of-fashion interstate conflicts are easier to end through negotiation than intrastate ones. "Only a quarter to a third of modern civil wars (including anticolonial wars) have found their way to negotiation, whereas more than half of modern interstate wars have done so," writes the editor (p. 3).

The theoretical framework in the Introduction and Conclusion—no surprise to those familiar with I. William Zartman's previous work—centers around the search for a "ripe moment," when warring parties are fatigued enough to seek either a face-saving formula for a way out or the middle ground between themselves and the opposing side(s). Compromises attractive to the mainstream make them emerge as valid spokespersons whose views are then weighty enough to isolate extremists.

So far so good. But civil wars have other complications. Negotiators must be able to deliver—a problematic assumption because today's rebels are often ragtag insurgents who refuse to respect in the trenches what their so-called leaders have supposedly agreed in Geneva or New York. Moreover, there must be a realistic plan to disarm insurgents and mechanisms that will allow politics to occur by a means other than war. This latter task is extremely complicated in civil wars, which is why peace is "elusive" and requires more than such gimmicks as new boundaries.

Anyone who has struggled to edit a coherent collection of cases and who has managed to set out a common framework that allows authors to ask and answer similar questions, thereby permitting meaningful comparisons, will be interested by this well-conceived volume. The contributors examine the characteristics of particular civil wars along three dimensions—the structural, the dynamic, and the tactical—and suggest alterations in strategies for better results by future negotiators and mediators.

Some authors are more successful than others, but the individual essays are almost all of uniformly high quality. Rather than superficially characterize the eleven cases, this reviewer wishes instead to spell out three lingering concerns about the volume. To be fair, the editor and contributors readily admit how much conceptual work remains before having a solid theory, particularly because until recently policymakers had emphasized counter-insurgency rather than negotiations.

First, the book is based on a normative preference for negotiation over war. However laudable it may be morally, this preference underpins two highly controversial assertions—"negotiation is the best policy for both parties in an internal conflict" (p. 3) and "there must be a way out of polarization" (p. 344). One complication is that there are many more than two warring parties in many of these cases as well as in an increasing number of civil wars. Thus, the dynamics are more complex, and more parties may mean fewer benefits. Moreover, stalemate (or a no-win situation crucial to "ripeness") is seen as a solution. In fact, however, swifter and unencumbered victory by one faction might actually entail less total suffering and more rapid progress toward peace. Those who have examined the woes of Bosnia and Liberia after half-hearted international interventions and protracted negotiations can perhaps be forgiven for wondering whether quick victories in 1991 for the Serbs and Charles Taylor would have entailed less carnage than what "successful" negotiations in Dayton and Accra have produced. In short, war may be preferred over negotiation.

Notions of stalemate, ripeness, mediation, and negotiation presume that warring parties eventually must live together and at some point realize that talking is more profitable than continued violence. There are a growing number of ethnic conflagrations in which it is foolhardy to think that the belligerents can reconcile their differences. Analysts are understandably hoping to keep alive the ideal of multiethnic societies. Alas, more uniform populations may be a prerequisite for halting some civil wars, rather than stalemate, recognition, and dialogue. For instance, internationally assisted ethnic cleansing of Greeks and Turks—not mediation and negotiation—has fostered two decades of peace in Cyprus. Dayton was finally possible because ethnically homogeneous swaths of territory had replaced multiethnic fabrics. This depressing and increasingly pertinent reality is poorly reflected in the cases (only Eritrea is an example of military victory).

Second, the book's focus on civil wars curiously fails to overcome the natural statecentric bias of international relations. It proceeds from the assumption that "one party (government) is strong and the other (insurgents) is weak" (p. 7). Asymmetry in power in internal armed conflicts and the asymmetry of the accompanying negotiating dynamics do not favor state authorities in many cases—Afghanistan and Lebanon are in the volume, but others, like Rwanda and Somalia, are not. The prevalence of weak, collapsing, and failed states in the post-Cold War era would argue for more attention to asymmetrical situations favoring nonstates.

Third—and here the gauntlet is thrown down but not as yet triumphantly picked up by the contributors to this volume or by others, including the reviewer—there is a pressing need to make more analytical sense of unlike cases. This volume attempts to bridge the gap between comparing apples and baseballs. Globalization and integration lend themselves to sustained theorizing because such trends as financial markets, communications advances, and cultural expansion are composed of similar or at least comparable events. In contrast, the empirical consequences of extreme fragmentation, by definition, tend to lead analysts toward sui generis descriptions.

*Elusive Peace* is an important step toward identifying the requirements for successful negotiations. It draws many salient lessons from recent successes and failures and prescribes methods for helping to get closer to the negotiating table in some civil wars. There is certainly no better volume about negotiating an end to the plague of our times.

**Germany Unified and Europe Transformed: A Study in Statecraft.** By Philip Zelikow and Condoleezza Rice. Cambridge, MA: Harvard University Press, 1995. 493p. $35.00.

Andrei S. Markovits, *University of California, Santa Cruz*

In terms of any of the numerous epistemologies and methodologies that currently constitute the motley fields of comparative politics or international relations, this book is less than ephemeral. However, as a work of diplomatic history it is nothing short of monumental. By virtue of having been active participants in the innermost circle of American decision making during that fateful year between September/October 1989 (the immediate prelude to the Berlin Wall's opening on November 9) and Germany's official unification on October 3, 1990, the authors offer us a gem of a study in two related ways. First, they draw on a bevy of primary documents from U.S., Russian, and German sources that have hitherto eluded everybody else and which, alas, will remain, at least in part, elusive to mere mortals for years, maybe even decades. Second, to the authors' great credit, they have succeeded in harnessing the richness of their detailed data to write a veritable page-turner. Anyone interested in recent politics (as opposed to political science) will find reading this book a truly rewarding experience.

The book has no real argument. Far from offering the overarching pattern of a forest, this volume does not even provide the particularity of the proverbial trees. Instead, its beauty lies in the leafy details of singular events that changed the world in which we live. Some of this study's most fascinating contributions lie buried in its more than 100 pages of single-spaced and small-print footnotes that convey the obvious meticulousness of the research. Simply put, this book offers an insider's look at the innermost workings of the top elites of the United States, the Soviet Union, West Germany, East Germany, Britain, and France in the forging of a united Germany. By dint of their role as leading analysts for the National Security Council under the Bush administration, both authors were "present at the creation" of the post–Cold War world. Thus, this book offers a deeply biased history from above, in that it concentrates almost exclusively on the activities of a handful of elite players, which it then proffers as the reason for the immense changes that happened. But given the authors' unique vantage point and their singular knowledge of precisely this world of elite politics, this bias is perfectly reasonable, indeed justified, since it would have been a waste of their resources (and the reader's time) to have the study properly complemented by a history "from below" as well. Differently put, we already have had excellent scholarship that has enlightened us about the development of opposition movements in the former East Germany, the rise of Solidarity in Poland, or the failure of Soviet-style economies to adapt to the exigencies of a global market as factors (among many others) that obviously contributed to the collapse of "real existing socialism." But only scholars like Zelikow and Rice who spent all their waking hours with the players themselves could tell us how Helmut Kohl chewed out Giulio Andreotti at a NATO meeting in late November 1989 when the latter raised certain objections about the European implications of German self-determination; or when Kohl's foreign minister Hans-Dietrich Genscher turned to his Italian colleague Gianni De Michelis in the all-important Ottawa meeting of early February 1990, telling him sharply, "You are not part of the game" (p. 193).

Rather than as a book of political science, one should see the Zelikow/Rice study as a fascinating play whose outcome one knows yet whose players one gets to meet only through the details of this study. The authors offer us three leading characters: George Bush, Helmut Kohl, and Mikhail Gorbachev. In the Zelikow/Rice dramatis personae it is quite evident that without George Bush's early, repeated, and enthusiastic engagement on behalf of German unification matters might have turned out quite differently. Then there is Helmut Kohl, who—following his instincts once again much more than the coordinates of conventional politics—seizes a crucial moment (Lenin style) and performs a fait accompli, which—with George Bush's unqualified backing—ultimately leads to Germany's reunification. Lastly, there is the tragic figure of Mikhail Gorbachev, who—though brilliant and even forceful at times—seems to recede and withdraw (not in Lenin style) from page to detailed page as the book reaches its end. Add to this the supporting cast of the late François Mitterand, Margaret Thatcher, James Baker, Eduard Shevardnadze, Genscher, Egon Krenz, Hans Modrow, and a few other American, German (East and West), and Soviet policymakers, and the book's characters are complete. To be sure, none of what Zelikow and Rice tell us about these players is surprising to anyone who has had even a vague familiarity with these events. Yet the fact that the authors acquaint us in such a reliable way with these figures renders our earlier knowledge of them much more vivid as well as valid. Thus, for example, Margaret Thatcher's and François Mitterand's antipathies for German unification have been known for years. Yet, the manner in which these antipathies were expressed, how they differed from each other, and the way these two leaders dealt with their respective antipathies are helpfully described and credibly presented by Zelikow and Rice. It is the detailed presentation of anecdotes, meetings, moods, late-night draft sessions, cables, telephone conversations, and other minutia that make this study a fine example of "thick description."

In an all-too-rare attempt to give their work broader theoretical relevance (this, after all, would not be too bold to expect on a more systematic basis from authors who are eminent political scientists at two of the nation's finest research universities), Zelikow and Rice reject what they call a "whig" (meaning a structural) interpretation of history in favor of a more accident-prone and contingent view of political change. Given the two authors' uniquely privileged vantage point in the case of these singular events that changed global politics in a matter of barely one calendar year, one can hardly fault them for such a view. Indeed, their book offers a convincing testimony for the powerful validity of just such an interpretation of historical change. Yet, as the great French historian Marc Bloch has taught us so poignantly, we can only understand "missteps" in history properly by looking beyond the actual events themselves and taking the structures that facilitated the events into consideration as well. Scholarship that focuses on structures without giving agency its due remains abstract and lifeless; writing, which in turn excludes structure in favor of agency and contingency, runs the danger of never leaving the realm of description. While Zelikow and Rice are to be congratulated for offering us a fine exemplar of just such descriptive scholarship, one would have hoped that these two eminent authors would have at least attempted a Blochian synthesis in their interpretation (as opposed to presentation) of what will surely remain a unique data set for years.

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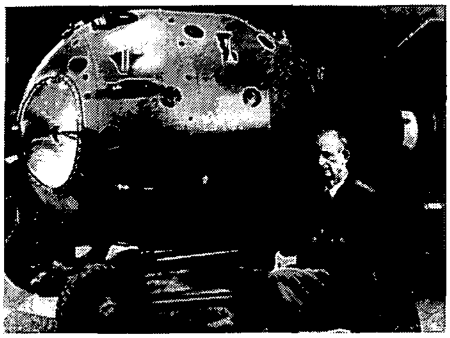

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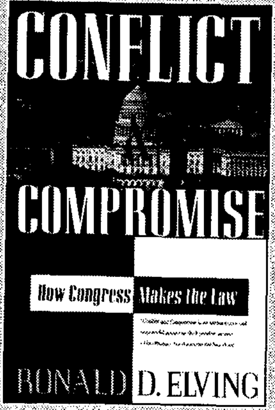
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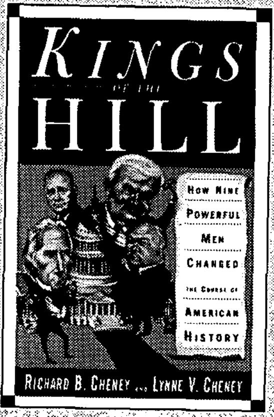
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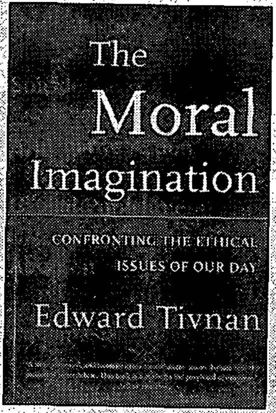
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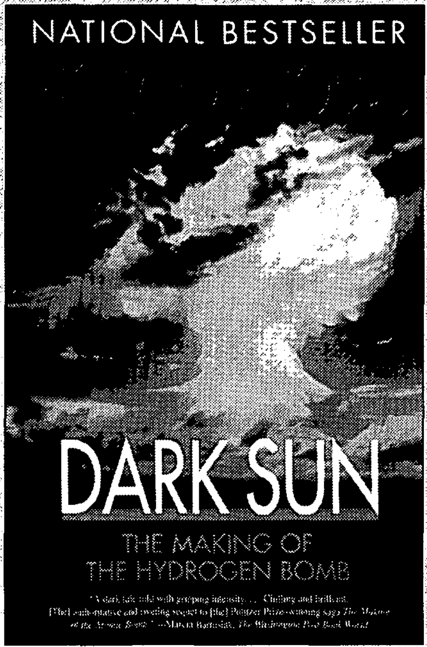
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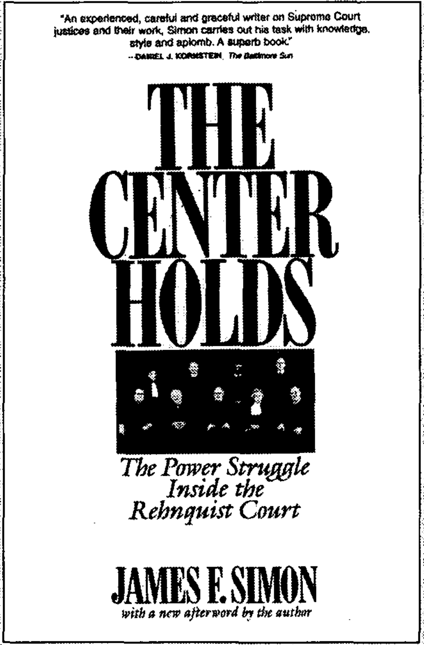
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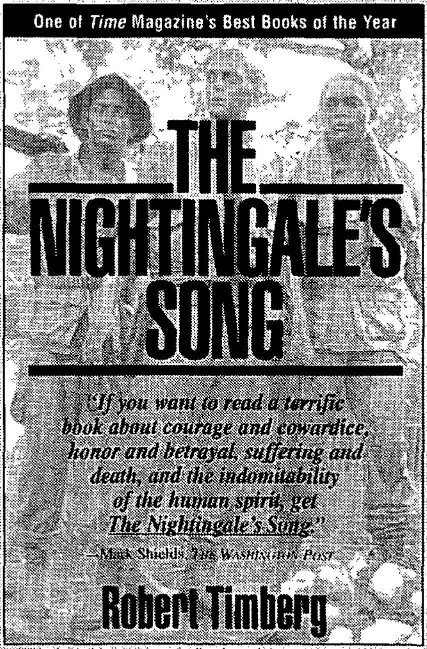
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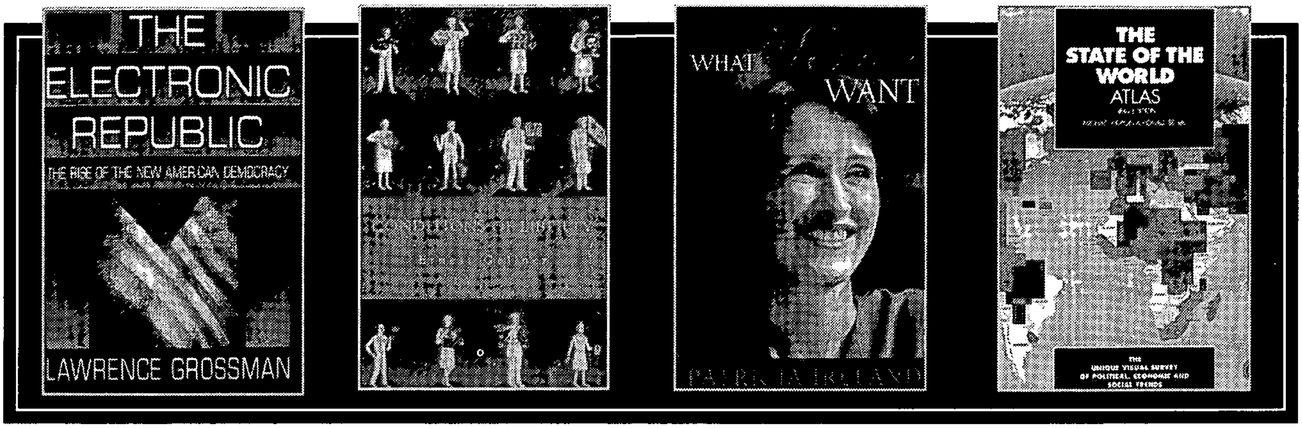
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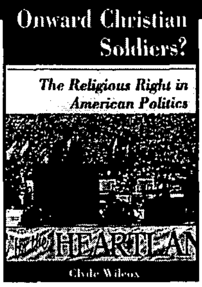

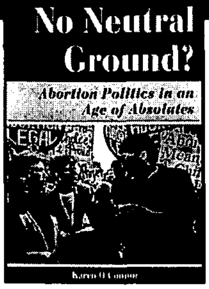

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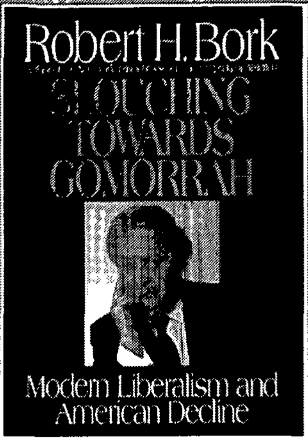
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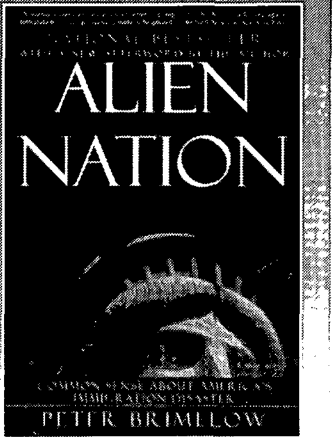

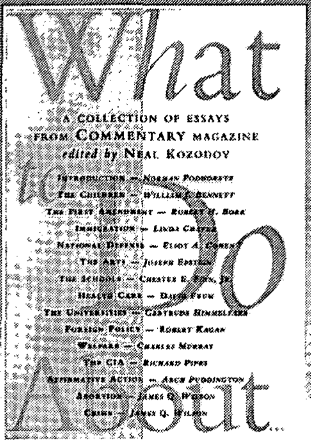

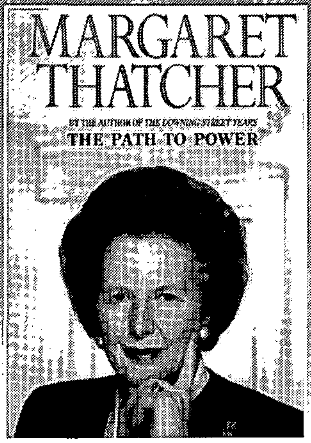

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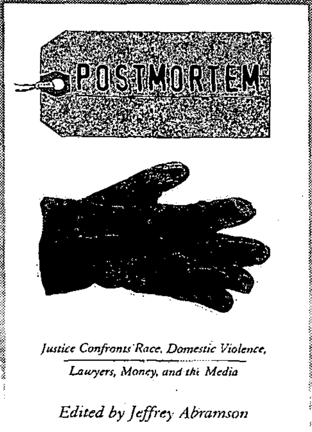
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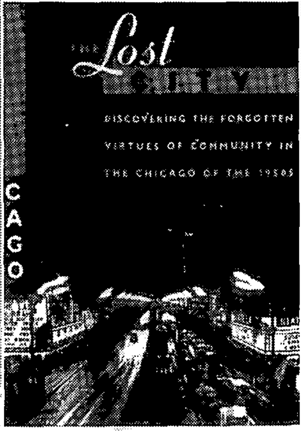
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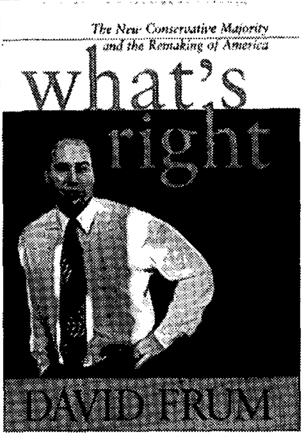

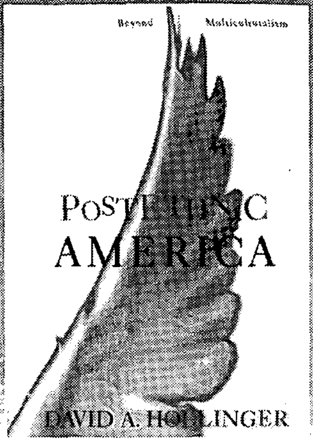

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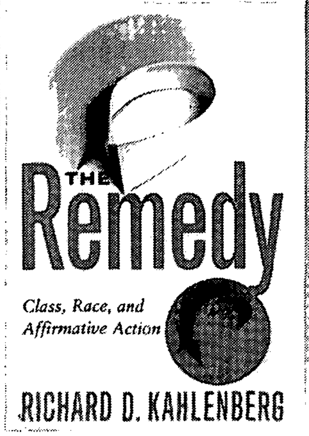

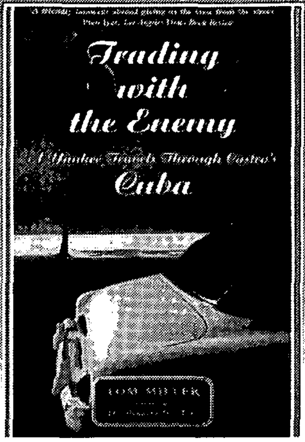

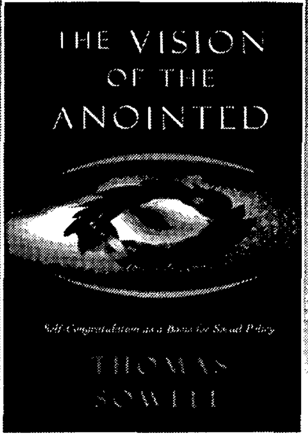
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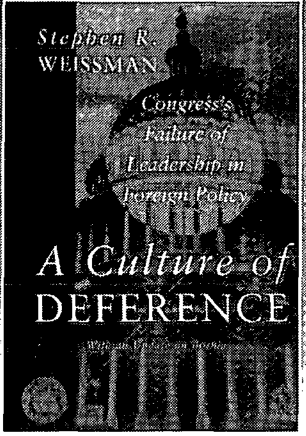

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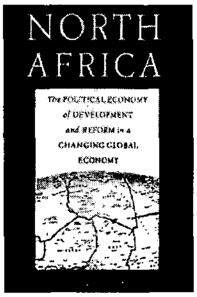
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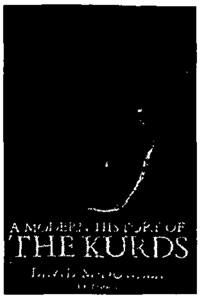

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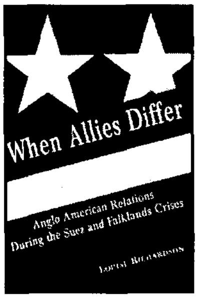
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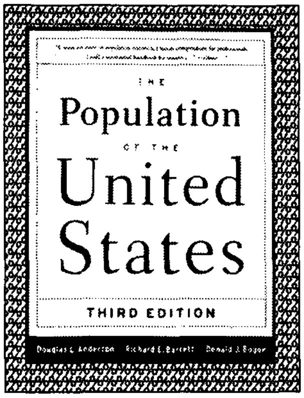

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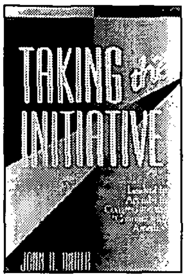

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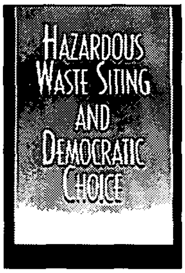

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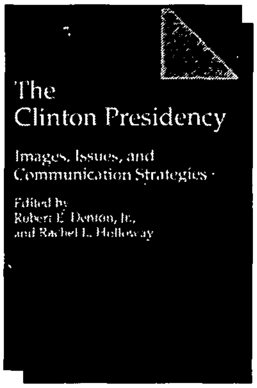

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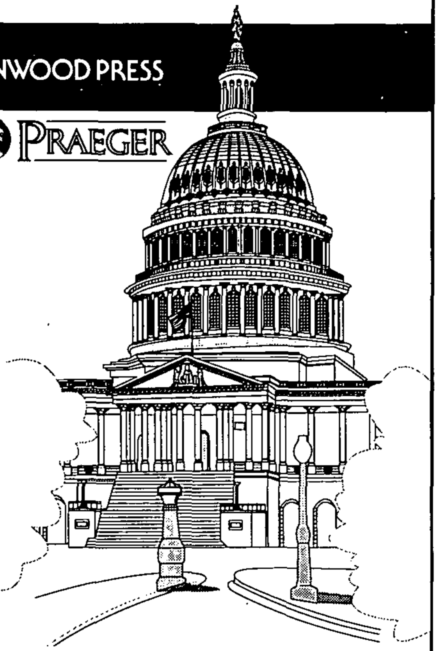

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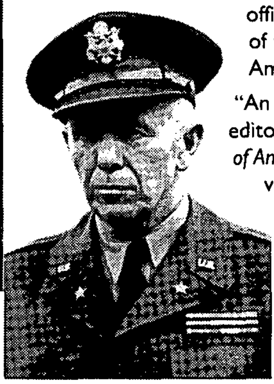

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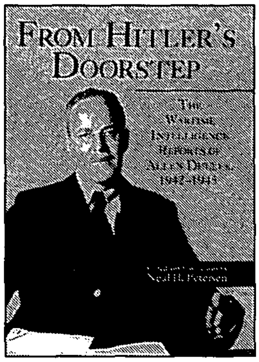

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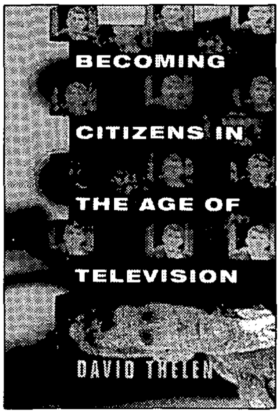

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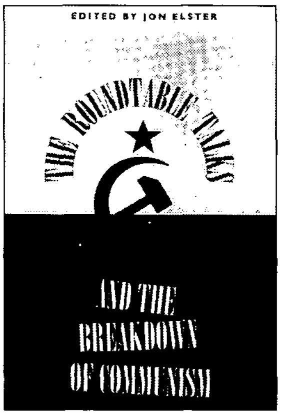

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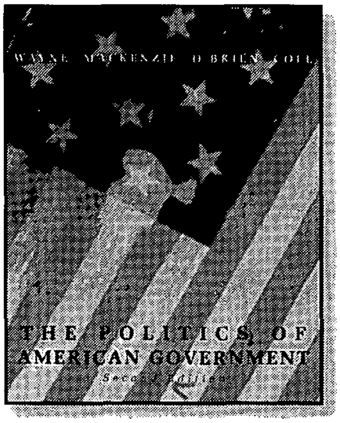
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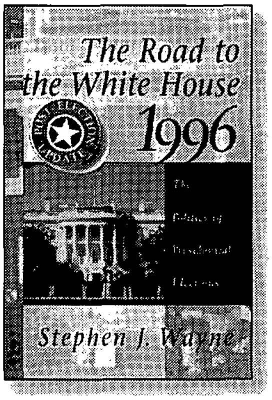
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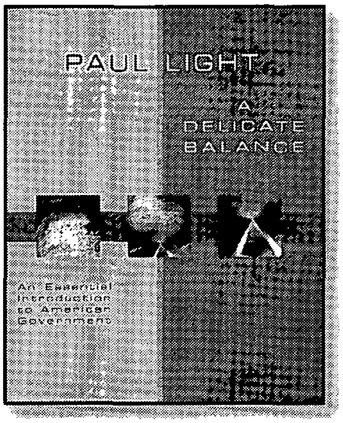
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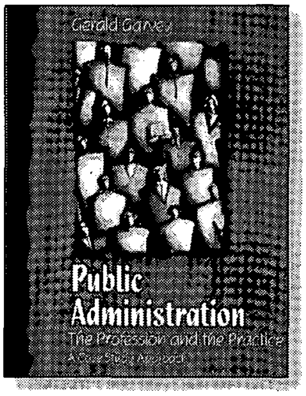
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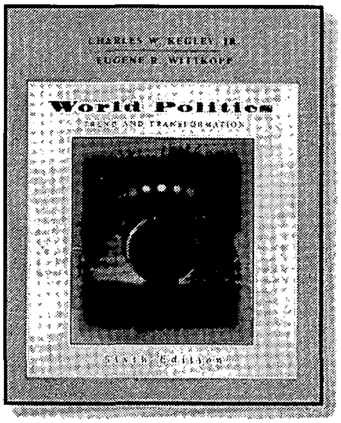
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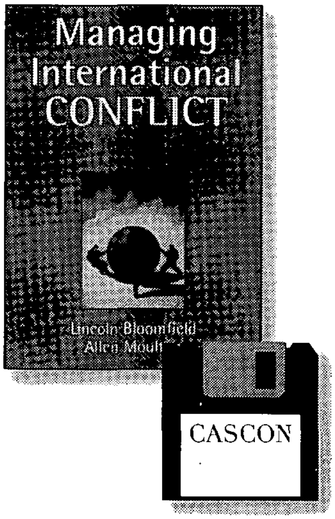
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# Table of Contents

EDITOR'S NOTES . . . . **viii**

ARTICLES

Political Inclusion and the Dynamics of Democratization
**John S. Dryzek** . . . . **475**

Hume's Constitutionalism and the Identity of Constitutional Democracy
**Robert A. Manzer** . . . . **488**

Serving God and Mammon: The Lockean Sympathy in Early American Political Thought
**Joshua Foa Dienstag** . . . . **497**

Assessing the Dyadic Nature of the Democratic Peace, 1918–88
**David L. Rousseau, Christopher Gelpi, Dan Reiter, and Paul K. Huth** . . . . **512**

The Costs of Coalition: Coalition Theories and Italian Governments
**Carol A. Mershon** . . . . **534**

Dynamics of Cosponsorship
**Daniel Kessler and Keith Krehbiel** . . . . **555**

The Dynamics of Aggregate Partisanship
**Janet M. Box-Steffensmeier and Renée M. Smith** . . . . **567**

Strategic Policy Considerations and Voting Fluidity on the Burger Court
**Forrest Maltzman and Paul Wahlbeck** . . . . **581**

RESEARCH NOTE

"Race Coding" and White Opposition to Welfare
**Martin Gilens** . . . . **593**

FORUM

History, Historiography, and Political Science: Multiple Historical Records and the Problem of Selection Bias
**Ian S. Lustick** . . . . **605**

BOOK REVIEWS

Political Theory . . . . **619**
American Politics . . . . **643**
Comparative Politics . . . . **665**
International Relations . . . . **690**